PEARL

The Journal of HEREDITY

GIANT BLUEBERRIES FROM 30 YEARS' BREEDING
(SEE PAGE 312)

Insanity and Genius Not Allied

Methods of Pedigree Analysis

"Lazy" Rice and Doughnut-Skulled Calves

VOLUME 29 AUGUST, 1938 NUMBER 8

The American Genetic Association

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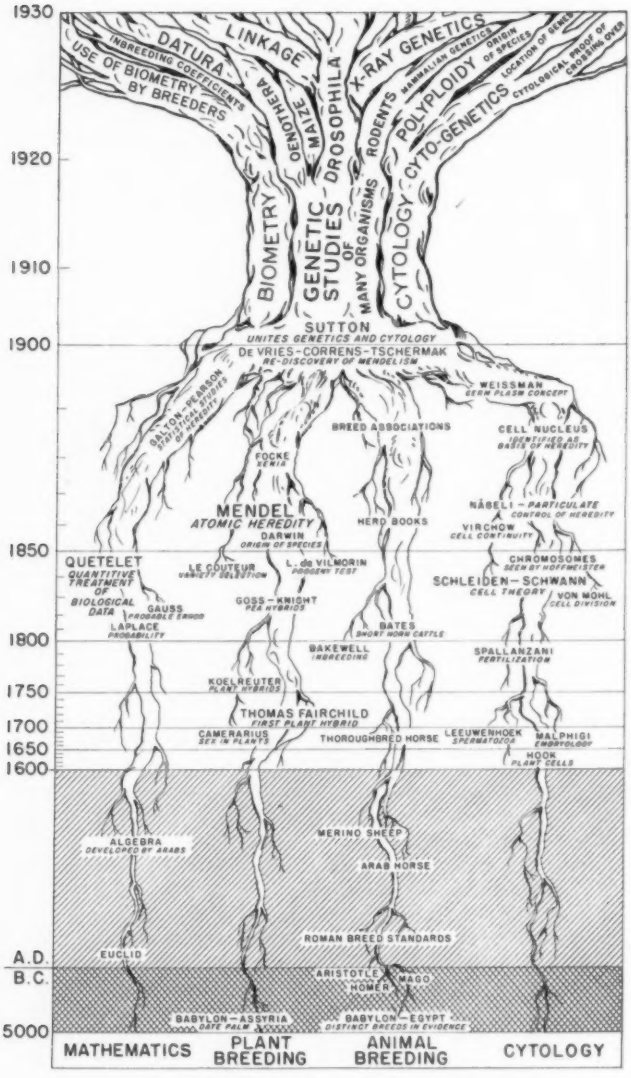


A PEDIGREE OF GENETICS

Frontispiece

A "Family Tree" of the development of genetics showing some of the more important events and a few key personalities in the history of modern genetic theory. Such a schematization is necessarily very sketchy and can do little more than suggest the rough outline. The chart was prepared by the Editor of the JOURNAL for inclusion in the 1937 Yearbook of Agriculture of the U. S. Department of Agriculture. (See review of the Yearbook in this issue, page 309.)

INSANITY AND GENIUS

E. M. East
Biological Laboratories, Harvard University

ONE of the most widespread and most firmly fixed of notions that pervade the human mind is the idea that genius is closely akin to insanity. It probably is found in the folklore of many peoples, since such a relationship can easily be read into the numerous tales which purport to form a foundation for the extensive beliefs in the madness of holy men. It goes back, in time, at least to ancient Greece, from which legends have come down to us that some of the celebrated philosophers were actually examined for their sanity.

The twentieth century writings on the subject appear to follow the groove cut by the work of Henry Maudsley, who, though he did much to alleviate the sufferings of the insane in English asylums, made some very queer statements on heredity in health and disease, which were accepted as authoritative merely because he was a physician. At the time of his activity, Francis Galton was the only real student of what is now known as genetics; and one might suppose that Maudsley would have paid some attention to Galton's work. On the contrary, it was neglected. Maudsley made assertions without supporting evidence, such as the following: "It is no exaggeration to say that there is hardly ever a man of genius who has not insanity or nervous disorder of some form in his family."

Followers of this line of thinking have copied Maudsley's assertions, presumably because they feel that they must make citations which simulate scientific authority; and they have also copied his dogmatic manner of writing out lists of supposedly insane geniuses, without troubling to give reasons for including the names. But the true incentive for their reactions is much simpler. It is almost a conditioned reflex. We ordinary people realize that we are not geniuses, and we invent a reason for not making the grade that is soothing to our egos. Our nervous systems are sound and healthy. We are mediocrities because we are well-balanced and do things like other people. All we needed to make us geniuses was an inherited or acquired mental taint, something that would have made our actions eccentric.

The writer has read several of these splenic outbursts against genius during the past year, as well as earlier; but the immediate stimulus for these remarks is a short debate on the desirability of sterilizing the feeble-minded, written by anonymous authors in the issue of the *Reader's Digest* for May, 1938. Mr. Pro makes the standard argument, bringing in the Kallikaks, the Jukes, and the statement of the late Justice Holmes that "three generations of imbeciles are enough." It is all sound, ripe stuff, and views the situation with proper alarm. Mr. Con, though his citations of genetic beliefs and the statistics on which they are assumed to be based cannot be accepted at par value, does make a really important point when he inquires whether the public should be satisfied with decisions of governmental agencies as to who should be sterilized. One gathers that even if legally constituted bodies are to be trusted with the lives and freedom and the public and private economics of the American people, nevertheless their average wisdom is too low for them to have anything to say regarding reproduction.

He may be right. We shall not take sides on this part of the question, but rather shall confine ourselves to the consideration of what Mr. Con takes to be a disturbing minor point. This point, in its more general outline, is the one met so frequently in anti-birth control writ-

ings as to whether our civilization is not being overly smart if it ever takes steps to interfere with reproduction. In the anti-birth control literature, the argument takes the form of defending maximum families on the grounds that otherwise the world may lose a genius. Since the only satisfactory reason for supposing that the first-born are handicapped is the obstetrical one of difficult parturition, and since such possible handicaps are so overweighed by the advantages of proper spacing of children, we need not enter into the matter here. Moreover, the bitter end of such an argument should be a defense of illegitimacy, for the parents of the super-genius, Leonardo da Vinci, were unmarried. And to give Mr. Con his due, he keeps to the narrower problem that we have already outlined. He says: "Manic-depressive symptoms, for example, are notably associated with the highest type of genius. If psychotic strains had always been sterilized to improve the human breed, civilization might never have benefited from the lives of such men as: Sir Isaac Newton, Michelangelo, Beethoven, Napoleon, Balzac, Hans Christian Andersen, Lord Byron, Goethe, Edgar Allen Poe, Strindberg, Dean Swift, Dostoievsky, Turgenev and Tolstoy. There are plenty of others in biographical dictionaries."

A Look at the Record

Mr. Con's assertions are typical of this kind of writing. First, he makes a broad allegation for which no evidence has ever been adduced. Next, he compiles a list of great men whom the reader is asked to accept as insane at one time or another during their lives, or, at the very least, as products of tainted stocks. The present writer has tried to get at the truth regarding the names listed by examining various standard biographies. Data were found for all except Strindberg.

Newton lived to be nearly 85, dying on March 20, 1727. The only indications that he was ever insane were that he never married, that he frequently was so absorbed with abstruse problems that his servants had to remind him to eat his meals, and that toward the latter part of his life, he wrote a religious commentary on the Apocalypse of Saint John which would have made a lasting reputation for a modern fundamentalist. True, there was a libellous rumor of insanity current on the continent about 1692, possibly due to misunderstandings of adherents of Leibnitz in the dispute as to priority in the discovery of calculus. Newton himself took little part in personal disputes, though he was an active combatant for the University of Cambridge when King James II tried to abridge its ancient rights. He was a calmly dignified and handsome man, even to the end, as the wonderful portrait painted by Vanderbank in 1725 testifies. At this time he was 83, and in that year, revised and enlarged the third edition of *Principia Mathematica*. Also, it must be noted that he was elected president of the Royal Society annually for the twenty-four years between 1703 and his death.

Michelangelo was another who reached a great age without showing notable deterioration of any kind. Death came in his ninetieth year (1564). Built apparently like the "one hosse shay," his sole symptom of insanity seems to have been his selfishness in doing the work of half a dozen men. Not satisfied with being the world's premier sculptor during his first fifty years of life, he became the greatest mural painter with his work on the Sistine Chapel (The Last Judgment was finished in 1541, at 66), and then proceeded to become a very celebrated architect by taking over and recasting the plans of Bramante and San Gallo for Saint Peter's at Rome.

Beethoven became deaf in his prime; and, as is so often the case after the onset of physical infirmities, tended to avoid society and to wax gloomier and more distrustful of his fellowmen.

Napoleon's psychological reactions certainly can not be judged by the standards that fit the average man. But the charge so often made by malevolent writers, that he was an epileptic, has no sound medical basis. True epilepsy would have been progressive in its effects; and

the symptoms could not have escaped Bourrienne, as they did, for the latter was not merely Napoleon's most acute biographer but was also, for eleven years, his secretary. It cannot be denied that the Little Corporal had some sort of functional disturbance at various times, which resulted in paroxysms; but the descriptions indicate that they were due to acute indigestion brought on by rapid eating.

Balzac's life shows no symptoms of insanity. Nor does that of Andersen. Andersen may be said to have showed particularly calm and normal reactions in facing extremely adverse conditions.

Byron appears to have had a convulsion at one time. In addition, his temper was often violent. But it must also be noted that he was extraordinarily calm in the face of physical danger. He died of some kind of rheumatic fever; and appears to have been rather susceptible to infectious bacteria.

Goethe's long life was normal enough in its psychology, except perhaps that, like many a lesser man, he had an extreme aversion to politics.

It is difficult for an unbiased person to see why Poe is so frequently listed among the abnormal geniuses. Poe was a great artist with a grade A mathematical mind. As such, he was somewhat hypersensitive to external conditions — to alcohol, as well as to impressions of beauty. One glass of wine was more than he could carry comfortably, and this fact made his reputation bad among a lot of people who could not make proper allowances.

Dean Swift appears to have been normal except, perhaps, in early life, though his reactions were not those ordinarily expected of clerics.

Dostoievsky was defective. He was a true epileptic.

Turgenev and Tolstoy were, by nature, gloomy, — and yet were excitable. Both did things that the ordinary man would not think of doing. Neither showed any real signs of insanity.

Gathering these facts together, one gets the idea that is always obtained by carefully studying one of the lists of supposedly insane geniuses, except that Mr. Con by accident included Dostoievsky. And Dostoievsky is the only true epileptic found by Havelock Ellis in his extended study of alleged epileptics among very noted men (Van Gogh may possibly be a second). One may conclude, therefore, that mental aberrations of certain types are not absolutely incompatible with genius, but also that a hospital for the insane is not a good place to search for supermen. It should also be understood that the genius is frequently a sensitive man and usually an extreme individualist. For these reasons, he should not be judged wholly by the standards that apply to mediocrities. That this is clearly the case, is seen in the story cited by Ellis (*Study of British Genius*), wherein Maudsley makes out that William Blake was certainly insane "if one is to believe the anecdote that Blake was accustomed to sitting naked in his summer house together with his wife." Ellis remarks that Blake had firm ideas concerning the beneficial effects of the sun's rays on the skin, and was thereby a century ahead of current medical therapy. Another thing is that geniuses often do several times as much work as ordinary men, and may thereby wear out their nervous systems early, or at least may fray them.

Disease and Old Age

Thus it is extremely difficult to make out a case for association between genius and insanity, even when one keeps to the selected lists made by the various Mr. Cons. Infectious diseases like syphilis, which may, in the end, affect the central nervous system, ought to be omitted entirely. So also should one probably omit senile dementia, if for no other reason, on account of the statistical difficulties involved. Again, one should handle biographical anecdotes with extreme caution. And even medical diagnoses should be accepted only in certain cases, having due regard for the time at which they were made. As a matter of fact, internal medicine is far from being an exact science, even today; and the internists, of whom there have

been many, who have made diagnoses from published records of symptoms of geniuses, have usually come to conflicting conclusions.

Among the Mr. Maudsleys and the Mr. Cons—who, if they have not made wholly unsupported statements to the effect that many, or even most, geniuses are neuropathic or of neuropathic stock, have at least made their judgments from unauthenticated anecdotes without any attempt to analyze them, scientifically—belong some otherwise competent people, such as Lombroso (*Man of Genius* and *The Female Offender*) and Nisbet (*Insanity of Genius*). Queerly enough, no modern geneticist has been sufficiently interested to make an extensive study of the question. This is probably because psychiatry has made such slow progress that the geneticist has little that is tangible to grasp. Straight experimental psychology has done something, if only in a gross way, in separating different grades of Amentia (undeveloped minds); but psychology has not been so successful in dealing with Dementia (deterioration of minds that had developed). Yet psychiatry has little to go on except psychological analysis, and here only during the present generation, — the study of pedigree records being thus practically prohibited. Histological study of lesions, the other main division of research, has produced little; in fact, it seems comparable, in design, to studying electricity by making cross sections of copper wires. Perhaps this is why several well-known psychiatrists spend their time on destructive criticism of the few results geneticists have obtained on feeble-mindedness, instead of getting busy on their own subject.

What the Real "Record" Shows

Aside from the types mentioned in the above paragraphs, however, three distinguished and competent men have investigated the inheritance of genius.

One of these men was Alphonse de Candolle, who wrote the *Histoire des sciences et des savants depuis deux siecles* in 1873. He says nothing about any neuropathology.

Another was Francis Galton, who wrote *Hereditary Genius,* in 1869, and *English Men of Science: their Nature and Nurture, in* 1874. He also says nothing about neuropathology, the word 'insanity' not even being mentioned in the first book. He does remark, however, on the prevalence of ill-health, to an amount greater than normally to be expected, in distinguished clergymen. In the second book, which included a study of 180 eminent British scientists, one-half of whom were between 50 and 65 years of age, Galton makes several statements indicating that the subjects were superabundantly healthy. "The leading scientific men are endowed with great energy." Again, "the typical man of science has been at full work from boyhood to old age, and has exuberant spirits and love of adventure in his short holidays, when the engine of his life runs free — temporarily detached from its laborious tasks." And still again: "the excellence of the health of the men in my list is remarkable, considering that the majority are of middle and many of advanced age."

In several ways, the most capable person to write on this subject has been Havelock Ellis, who published a monumental *Study of British Genius,* in 1904, and returned to the matter later, in several shorter essays, in which the subjects were not confined to the British Empire. Ellis, besides having the scientific temperament and being conversant with many of the subtleties of statistical method, had the advantage of a sound medical training. Further, he is one of the greatest psychologists and psychiatrists of our day, and has been intensely interested, for years, in the precise subject under discussion.

The first important finding of Ellis is that while some distinguished men did come from apparently neurotic strains, the percentage was low. The proportion of persons of genius with one insane parent, even when senile dementia is included, is less than 2 per cent, — thus being no greater than that found in the general population of either Great

Britain or the United States. This shows that the parents of geniuses virtually all would have escaped sterilization in even the harshest eugenic courts.

As to men of genius themselves, Ellis found statistical evidence of the prevalence of gouty conditions, about which one need not be concerned, since they are the result of ill-advised dietary habits and infections, and perhaps a slight indication of greater than normal expectancy of angina pectoris.

As to insanity of all kinds, Ellis found that the ascertainable number of cases out of 1,030 eminent persons investigated was 44. This proportion of 4.2 per cent he estimates to be about double that in the unselected population. The carefulness with which Ellis traced out the rumors and old wives' tales should be examined in the original writings to get the full significance of the work and expert judgment involved; and one may even accept the results as they stand without admitting any statistically significant association between genius and insanity. At the same time, one gets the idea that Ellis leaned over backwards to give the best case possible for an association between mental disease and genius, except in diagnoses where his professional pride was involved and where he was as 'hard-boiled' as a politician. It may be suggested (1) that insanity due to syphilis or other infectious diseases might better be omitted unless there was evidence that conditions interfered with intellectual performance; (2) that senile dementia should always be omitted unless something more than old age breakdown; (3) that careful scrutiny of the subjects involved gives reasons for eliminating approximately one-third because they were geniuses only by courtesy, and (4) that the estimate of comparable cases in the general population is nearer 5 per cent than 2 per cent. In connection with the last matter, it should be noted that the present writer made a careful study of insanity in the United States and also in England from all available statistics, some years ago, and has estimated that in both countries 5 per cent is a reasonable figure, apart from cases of amentia.

Thus it is seen that where one collates the work of the most competent investigators on the possibility of a relation between insanity and genius, the conclusion is unavoidable that none exists.

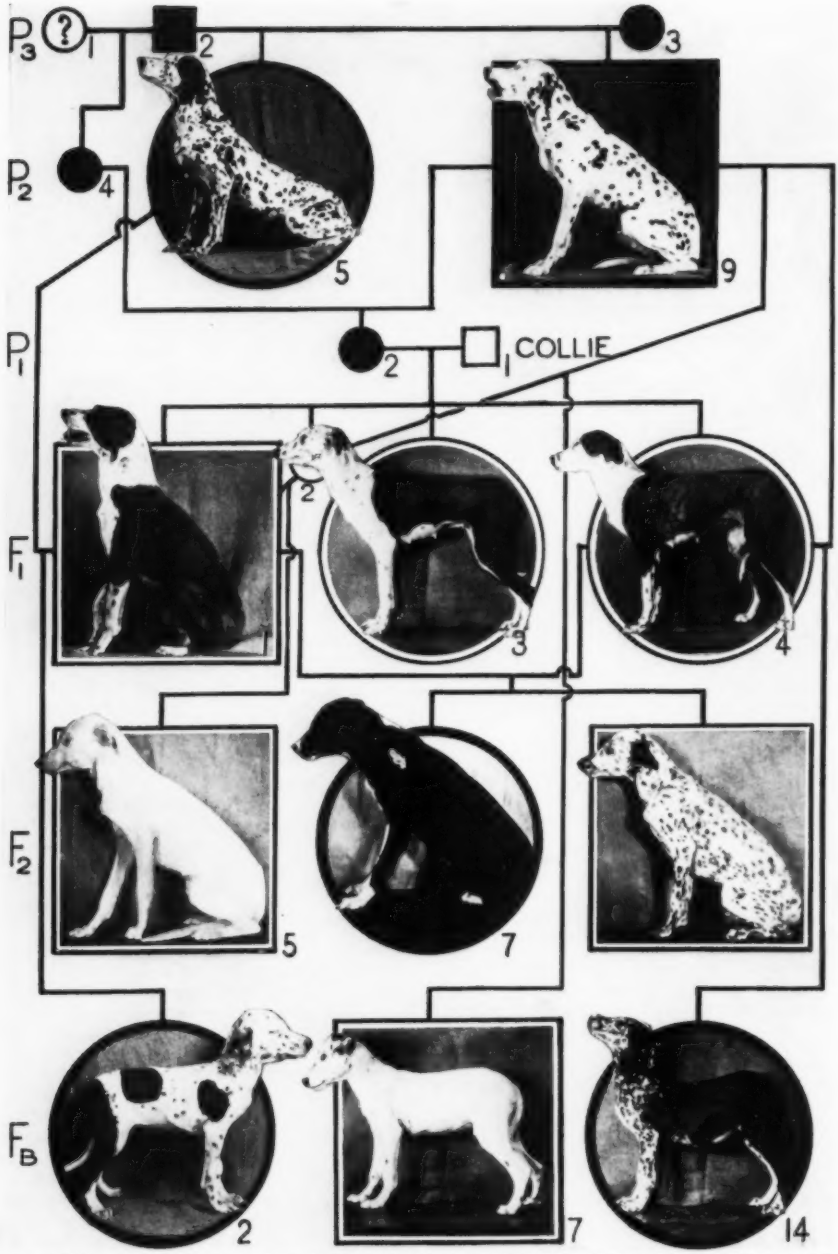
◆

Back-to-the-Farm Movement Continues

THE back-to-the-farm movement continues undiminished according to indications of a special 1938 survey just released. In describing the results, William L. Austin, Director of the Bureau of the Census, Department of Commerce, says that one in every nine of the 3,000 farms enumerated reported persons who had not resided upon a farm five years previously. Farms in selected counties in 40 States were included in the project, the field work of which was done by the statisticians of the Division of Crop and Livestock Estimates, Bureau of Agricultural Economics, U. S. Department of Agriculture, in cooperation with the Bureau of the Census.

The proportion of this migration is about the same as for the selected counties shown by the regular Farm Census three years earlier. The average number of persons for each enumeration year was 3.0. In nearly half of the farms involved three or more persons were recorded in the farm migration and their total represented nearly three-fourths of the entire farmward trek. The same general situation prevailed in the selected counties in 1935. This and other internal evidence indicate that the sample, although very small, is representative to a fair degree.

The 1938 survey covered the movement to farms during the period 1935 to 1938, while the complete Farm Census of 1935 covered the movement during the period 1930 to 1935. Thus, as the two years 1933 and 1934 were common to each period covered, the more recent, but very limited, survey shows that the farmward movement of city people to the wide open spaces during 1935, 1936, and 1937 was probably of about the same magnitude as that during 1930, 1931, and 1932.



A PHOTO PEDIGREE OF INHERITANCE OF COAT COLOR AND URIC ACID EXCRETION IN DOGS

Figure 1

Photographs of some of the dogs used in the experiment on inheritance of uric acid-allantoin excretion. For the full pedigree chart see Figure 2. The Dalmatian breed differs from most dogs in excreting uric acid instead of allantoin, a compound in which metabolic degradation has been carried one step farther. The circle-square symbols outlined in solid black indicate "high uric acid excretion"; the light symbols, "low uric acid excretion." Why one breed of dogs should differ in this respect from all others thus far studied remains something of a mystery.

THE INHERITANCE OF "HIGH URIC ACID EXCRETION" IN DOGS

HARRY C. TRIMBLE and CLYDE E. KEELER*

With the assistance of D. J. Mullane and Harry Winer

THE metabolism of purines is known to yield different end-products in different species of mammals. In some, uric acid ($C_5H_4N_4O_3$) is the substance excreted in largest quantities; in others the metabolic degradation is carried further and allantoin ($C_4H_6N_4O_3$) is the principal product eliminated. In 1914, Hunter and Givens[1] recorded very extensive observations upon the purine metabolism of mammalian species which had not been investigated previously, and discussed and classified all data available at that time. In their summary man and the chimpanzee were listed as the species which metabolize purines to *uric acid* and then excrete that substance, whereas a guenon monkey (*Cercopithicus Callitrichus*), various ungulates, marsupials, rodents and carnivores were included in the group which excrete *allantoin* as the *principal* end-product of purine metabolism. In 1920,[2] the house mouse and an Indian elephant were added to the latter group.

In 1916, an important exception to this classification was discovered by S. R. Benedict,[3] when he found a dog whose purine metabolism did not proceed in the manner supposedly established for carnivores. This animal excreted in its urine almost as much uric acid per day as does an adult man. Upon a diet which was purine-free, uric acid was eliminated at the rate of 36 mg. per kg. of the animal's weight per day. This rate remained constant even with a large variation (2 to 24 gm.) in the protein nitrogen of the diet. This animal belonged to the Dalmatian breed. Four other animals of the same breed were examined by Benedict. Numerical data for these animals were not recorded, but the statement was made that three of the four individuals excreted large quantities of uric acid while the one exception "obviously was not of very pure breed."

These observations indicating that one breed of dogs might have a manner of metabolizing purines different not only from all other known breeds but from all other members of the same taxonomic family aroused the immediate interest of a number of investigators. Wells[4] investigated the distribution of the enzymes capable of acting upon purines among the tissues of one Dalmatian dog. He detected the presence of uricase in the liver of this animal. In this important respect the Dalmatian did not differ from dogs of other breeds. Thus its unique capacity for excreting uric acid could not be explained by absence of the enzyme necessary for oxidizing this substance on to allantoin.

Onslow[5] undertook an investigation of the heritability of uric acid excretion by hybrid descendants of a Dalmatian dog. He mated a female of this breed with a pure white terrier and obtained a male and a female hybrid. When these latter were mated they produced an F_2 litter consisting of five females and one male. Observations were made upon the excretion of uric acid, allantoin and total nitrogen of all individuals of the series, except the F_2 male, which lived only several weeks. The terrier ancestor, its two hybrid descendants and four of its fe-

*The Biochemical Laboratory of Harvard Medical School and the Howe Laboratory of Ophthalmology, Harvard Medical School respectively.

male grandchildren excreted allantoin as the *principal* purine end-product. The uric acid nitrogen in their urines was only 0.2 to 0.4 per cent of the total nitrogen excreted. Their output of allantoin nitrogen was recorded as 16 to 21 per cent of the total nitrogen. Thus their uricolytic indices* were 98-99. The Dalmatian ancestor and *one* female descendant in the F_2 generation excreted much more uric acid. This constituted 2 to 3 per cent of the total nitrogen of their urine. Their output of allantoin nitrogen was 13 and 17 per cent of the total nitrogen. Their uricolytic indices were 84 and 85 respectively. (Numerous observations in this laboratory have never yielded a uricolytic index higher than 58 for a pure-blood Dalmatian, nor an allantoin output higher than 5 per cent of the total nitrogen.) On the basis of these observations Onslow remarked:

"In regard to the uric acid excretion, it is clear, from the results in the second generation, that there has been a segregation of the character involving lack of this power. Apart from this outstanding fact, *it is difficult to postulate the exact course of inheritance from the data given by this one family alone.* The results, however, suggest that, in main outline, the grandfather terrier, (probably of mixed breed) carried a dominant factor for destruction of uric acid; also a factor for the inhibition of spotting which is not always entirely dominant in the hybrid. If it is assumed that these two factors segregate themselves independently, then it is a simple two-factor Mendelian case (*though there are without doubt subsidiary complications*)" (Italics ours).

Since Onslow had pointed out that his observations were based upon a very few animals and had been terminated with the principal genetic questions still unsettled, it seemed desirable that the inheritance of this interesting biochemical characteristic should be given further study. This appeared particularly important because heritable characteristics which are chemically measurable are so few.

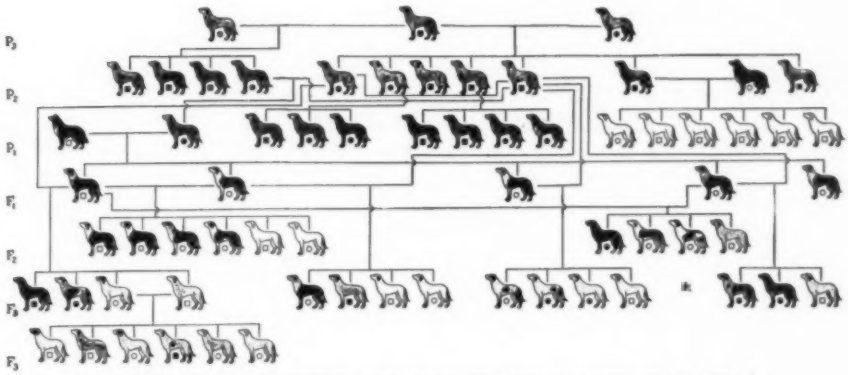
Experimental

The observations to be described were made upon a family of Dalmatian dogs, upon two collie dogs and upon the hybrid descendants of Dalmatians and collies. The Dalmatians used in producing the hybrids were raised in our laboratory. They were from a Dalmatian stock whose uric acid excretion had been tested by quantitative chemical methods and proved to be equal to that recorded by Benedict in his original observations upon the breed. The collies also had been tested upon numerous occasions and were known to be entirely typical of that breed. The first family of hybrids resulted from the mating of a Dalmatian dog (P_210) and a collie bitch (P_211). (See pedigree chart—Figure 2.) Later a Dalmatian bitch (P_12) was mated to a collie dog (P_11). Hybrid descendants in F_1, F_2 and F_3 as well as F_B generations were obtained. The uric acid excretion capacities of the animals studied will be discussed in later paragraphs.

Chemical measurements of the inherited characters. Purine-free diets were fed to all animals while their excretion of uric acid was being measured. In most cases the specimens analyzed represented the metabolism of a full 24 hour period. In general our results represent the average of several such tests. In classifying our animals the criterion used was the number of milligrams of uric acid per kilo. of animal weight excreted per 24 hours. The details of the chemical techniques and findings will be recorded elsewhere by the senior author. At this place it is sufficient to state that the numerical ratings obtained were not influenced to a significant extent by variations in

*Uricolytic index $= \dfrac{\text{allantoin } N}{\text{uric acid } N + \text{allantoin } N} \times 100$

It is obvious that the numerical values of the index will approach 100 when the quantity of uric acid involved is low and will decrease as the output of uric acid rises with respect to allantoin.



INHERITANCE OF SPOTTING AND OF URIC ACID SECRETION

Figure 2

The dogs used in this study trace to three Dalmatians (P_3), two of whom were shown by test to be "high uric acid excretors"; the third (female on the left) was not tested chemically. In the P_2 and P_1 generations, offspring of these three dogs were mated to two collies—a "low uric acid" breed. The first hybrid generation (F_1) all resembled the Collie parents in purine metabolism.

the nitrogen content of the diet, provided purines were excluded. All animals studied fell into one or the other of two groups. Those whose uric acid excretion ranged from 4 to 10 mg. per kg. per day constituted one group. Hereafter these are designated as "low-producers" of uric acid or stated to have "low uric acid excretion." Those whose uric acid excretion was 28 mg. per kg. per day or higher constituted the other group and will be designated as "high-producers" or stated to have "high uric acid excretion."

Onslow, for his rating of hybrid animals, relied upon two chemical methods. (a) He determined the ratio of the uric acid nitrogen to that of the total nitrogen of the urine. (b) He calculated the uricolytic index (which is the ratio of allantoin nitrogen to the combined output of uric acid nitrogen and allantoin nitrogen). The numerical values of both his criteria could have been influenced by increases and decreases in the nitrogen content of the diet. In view of Onslow's description of the efforts to provide similarity of diets when his hybrids were being compared, it is unlikely that this factor did influence his results significantly.

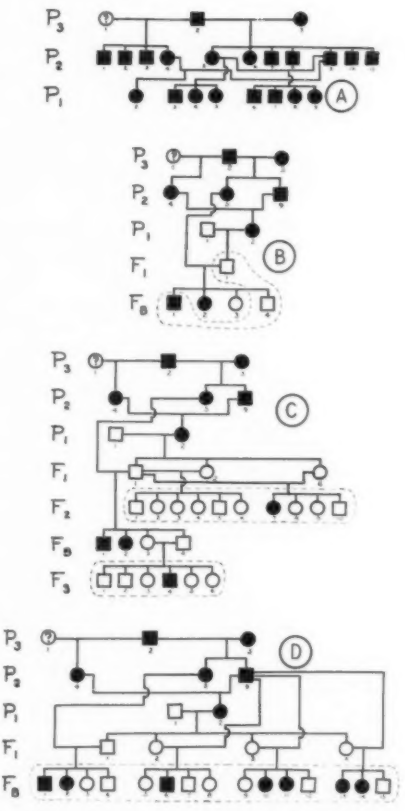
Inheritance

The condition of "high uric acid excretion" in dogs presents a number of genetic questions. Some of these are: (I) Is it hereditary? (II) If hereditary, what is the mode of its inheritance? Is it a dominant requiring but one mutant member of a pair of genes to insure expression of its character, or is it recessive in that both members of a pair of genes or allels must be mutant in order to determine "high uric acid excretion" in the individual? Is it completely dominant or completely recessive as are so many mammalian characters? Is it incompletely dominant or incompletely recessive in that hybrids are intermediate with respect to the quantity of uric acid excreted? (III) Is it sex-linked like haemophilia, being transmitted from a male to his daughters' sons in outcrosses, because its determining gene happens to reside in the x-chromosome? (IV) Or again, does "high uric acid excretion" depend upon multiple genetic factors, as do such quantitative characters as butterfat content in the milk of cattle? (V) If the character should turn out to be of a simple Mendelian nature involv-

ing a single pair of allels (dominant, recessive or sex-linked), is it genetically associated in any way with the "polka dot" or "harlequin" type of spotting characteristic of the Dalmatian? That is to say, is the gene or gene pair determining "high uric acid excretion" also responsible for producing the spotting pattern with which it has been found almost invariably in the past, just as the gene for dominant spotting in the laboratory mouse is known to produce in addition a lethal type of anemia when this gene is present in two doses? (VI) If the gene for "high uric acid excretion" is not itself responsible for the Dalmatian spotting pattern, does the gene for "high uric acid excretion" reside in the same chromosome with that for Dalmatian spotting? That is, are they genetically linked? Such a situation would simplify the problem of why one breed of dogs should differ from others in a character which, being unknown until recently, could not be selected for.

We have collected breeding data sufficient to answer in a very definite fashion these genetic questions which Onslow's report had left either in doubt or unanswered. These are discussed individually in the following sections.

(I) *Heritability of uric and excretion.* When an anomaly is confined almost exclusively to a particular strain or family it is quite likely to be inherited. In such circumstances both members of a pair of animals from this strain are purebred for the hereditary factors or genes determining the anomaly, and all offspring of such pairs should bear the anomaly. In this connection it should be recalled that Benedict[3] found "high uric acid excretion" in four out of five of the Dalmatians he examined. He made no statement regarding any relationship between his animals. Wells,[4] Onslow,[5] Folin, Berglund and Derick[6] and Krafka[7] have found "high uric acid excretion" in single Dalmatians representing strains of the breed which were widely separated geographically. Together these observations have constituted the basis for the belief that "high uric acid excretion" is a unique characteristic of the Dalmatian breed.



DETAILED PEDIGREE CHARTS

Figure 3

Sections from the general pedigree chart showing special features which are in the text. The data all fit the hypothesis that this metabolic difference is controlled by a single gene, "low uric acid excretion" being dominant.

We have information about the uric acid excretion of individuals of three generations in a particular family of Dalmations. (See pedigree, Figure 3*A*.) Twenty-one dogs have been tested in these three generations and all were found to be "high-producers" of uric acid. Although we do not have complete information upon all members of

this family, the data that we do have are quite in keeping with the interpretation that this family was purebred for the possession of the "high uric acid excretion" character. When we consider that many dogs not of the Dalmatian breed have been tested and none has been found to possess the character, these facts become more convincing and indicate that the "high acid excretion" is definitely a characteristic of the Dalmatian breed and is inherited in this breed.

(II) *Mode of inheritance.* To test the actual mode of inheritance, a genetic outcross must be made to a stock having "low uric acid excretion." This has been done in two ways. (See complete pedigree—Figure 2.)

In one test a collie bitch ($P_2$11) bearing "low uric acid excretion" was mated to a Dalmatian dog ($P_2$10) bearing the "high uric acid excretion" character. The litter (P_1: nos. 10-15) consisted of five males and one female, all of which had "low uric acid excretion." The quantity of uric acid excreted per day by the hybrids was only slightly higher than that of the collie mother, whereas it will be remembered that the amount produced by the purebred Dalmatians is about 7-10 times that of collies and other non-Dalmatian dogs. The spotting patterns for pups of this litter were not recorded in detail sufficient for an analysis and hence we have omitted this feature from our chart of this family.

In a second experiment a Dalmatian bitch ($P_1$2), having "high uric acid excretion," was mated to a collie dog ($P_1$1), having "low uric acid excretion." Their litter (F_1: nos. 1-5) consisted of one male and four females, all of which had "low uric acid excretion."

In the two experiments cited eleven outcross hybrids were produced. All had uric acid excretion of 10 mg. per kg. per day or lower. In respect to this character they followed the behaviour of their collie parents and were distinctly different from their Dalmatian parents who excreted 28 mg. or more for each kg. of body weight per day. The fact that the quantity of uric acid excreted is reduced so sharply in F_1 hybrids of Dalmatian outcrosses is sufficient to prove that the "high uric acid excreting" character is due to almost completely recessive factors of some kind. In addition, we have found that the uric acid excretion of our F_2 hybrids and of our backcross hybrids has been either below 10 mg. or above 30 mg. per kg. of body weight per day. Thus in our experience the contrasting characters of "low uric acid excretion" and "high uric acid excretion" are separated sharply in that there is *no overlapping of classes and in that there are no intermediate grades.*

(III) *Tests for sex-linkage.* Can "high uric acid excretion" be a sex-linked recessive character? The fact that both male and female Dalmatians exhibit the character tells us nothing, because any stock can be made purebred for a recessive-sex-linked character. In such a case all males and all females of the stock will exhibit it, as was true for our family of Dalmatians (see earlier paragraphs). The possibility of "high uric acid excretion" being a sex-linked character is ruled out, however, on the following basis. If it were sex-linked all hybrid males from the outcross of Dalmatian females to unrelated males should have "high uric acid excretion." Such hybrid males would receive from their mother an x-chromosome carrying the recessive mutant gene or genes for "high uric acid excretion," but they would receive from their father a y-chromosome lacking the normal allels of those mutant genes. This would allow the mutant genes to express themselves in all male offspring of the mutant mother, no matter to what kind of a male she may have been mated.

From females having "high uric acid excretion" and males with "low uric acid excretion" there were produced three hybrid males, two of which had "low uric acid excretion" (see pedigree section B). Hence the character is not sex-linked.

LIGHT AND NORMAL DALMATIAN SPOTTING

Figure 4

In this study some of the Dalmation segregates had diffuse spots in which white hairs were interspersed, as in the dog in front. The normal Dalmatian spots are solid black, as is shown in the other dog. The light spotting was associated with low uric acid excretion in the animals observed in these experiments.

(IV) *Tests for number of genes involved.* To determine the number of pairs of recessive genes responsible for "high excretion of uric acid," test cross populations must be produced either by mating together first generation hybrids (F_1) (see pedigree section C), or by backcrossing first generation hybrids (F_1) to P_1 parents possessing the recessive anomaly. Both of these tests have been made.

In tests of the first type the hybrid male ($F_1 1$) was crossed to two F_1 females ($F_1 2$ and $F_1 4$) (see pedigree section C). Also an F_3 litter was produced by crossing together two backcross hybrids $F_B 3$ and $F_B 4$. This latter litter (also in pedigree section C) was genetically comparable with the two straight F_2 litters because each parent ($F_B 3$ and $F_B 4$) must have received the recessive gene or genes for "high uric acid excretion" from their Dalmatian mother ($P_2 5$) who was also their father's paternal aunt. These pups are classified in Table I. The "low": "high" ratio actually observed was 14 "low": 2 "high." Upon the basis of one pair of recessive genes the ratio expected is 12 "low": 4 "high." Upon the basis of two pairs of recessive genes the ratio expected is 15 "low": 1 "high." Upon the basis of three pairs of recessive genes the ratio expected is 15.79 "low": 0.21 "high." It is apparent at once that "high uric acid excretion" is dependent upon the presence of not more than two pairs of recessive genes. The chances are greater that the ratio observed in our experiment represents the recombination of one pair of genes. However, these F_2 type data of themselves do not constitute conclusive evidence.

In the second type test we have backcrossed three F_1 females ($F_1$2, $F_1$3, $F_1$4) to their maternal grandfather ($P_2$9). In addition, the F_1 male ($F_1$1) has been backcrossed to his own maternal grandfather's sister ($P_2$5). These matings (see pedigree section D) gave us a total of four comparable backcross litters, the individuals of which we have classified in Table II. The "low": "high" ratio found there is 8 "low": 7 "high." The "low": "high" ratio expected upon the basis of one pair of recessive genes is 7.5 "low": 7.5 "high." The "low": "high" ratio expected upon the basis of two recessive genes is 11.25 "low": 3.75 "high." Obviously, our data are best interpreted upon the basis of the free segregation and random recombination of one pair of recessive autosomal genes. That is to say, the "high uric acid excretion" of Dalmations is inherited as a simple recessive Mendelizing unit-character.

(V) *Tests for the manifestation of the "harlequin" gene.* Is the "high uric acid excretion" produced by the same gene setup that produces the "polka dot" spotting characteristic of Dalmatians? Our data indicate that it is not so produced.

In our experiments eight backcross (F_B) pups (nos. 3, 4, 6, 7, 8, 11, 12, 15), three F_2 pups (nos. 5, 6 and 10) and two F_3 pups (nos. 5 and 6) had the Dalmatian distribution of spots. Our interpretation, therefore, is that these bore two genes for Dalmatian spotting. Of these 13 animals, only two (F_B6 and F_B11) had "high uric acid excretion." This fact is conclusive evidence that "high uric acid excetion" is not determined by the same pair of genes that produces the harlequin type of spotting found in purebred Dalmatians.

Pigmentation and uric acid excretion. The Dalmatians which eventually are most prized by fanciers are born with a coat which appears entirely white. After about ten days the spotting pattern begins to make its appearance and within three weeks becomes definitely evident. Among the offspring of pedigreed animals there may appear pups marked at birth with very black areas of variable size. (Since such offspring are not desired by the fancier, it is difficult to learn how frequently they occur.) The pigmented areas may involve the region of the ears or the eyes or both and may appear less frequently on the flank. These larger pigmented areas are commonly known as "patches" in contradistinction to the "spots" which appear later and which are smaller and more nearly circular in shape. The spots of the most prized animals are clearly defined and intensely black. These are sharply delimited in that there is no diffusion of pigment into the adjacent white areas. We do not know of any cases in which pedigreed Dalmations have white hairs interspersed among the black hairs of their pigmented spots. However, among the progeny of our crosses there have appeared animals in which some white hairs were distributed rather evenly among the black of pigmented spots. These white hairs did not invade the larger black areas of the pigmented saddles and "patches" present at birth in some of our hybrids. Such animals bearing white hairs diffused in their spots have been consistently "low-producers" of uric acid, while those animals which have had "high uric acid production" have had uniform black pigment in their spots. (See photographs.)

(VI) *Linkage tests.* If "high uric acid excretion" and Dalmatian type spotting are not effects produced by the same gene we may ask whether they reside in the same chromosome. That is, are they genetically linked? Our analysis of this point is slightly complicated by the fact that the collie dog figuring in our crosses bore the recessive type of spotting so frequently found in that breed, having white muzzle, neck, belly, feet, socks and tip of the tail. This complication merely means greater variation of spotting expression in individuals bearing one or both genes for collie spotting, in addition to one or both genes for Dalmatian spotting.

In analyzing our data we have considered that an animal had *two* genes

for the Dalmatian spotting pattern when it had a black patch on the head or the ears only. We have assumed the presence of but *one* gene for the Dalmatian spotting pattern when the animal had body patches of black in addition to the patches on the head. Equal numbers of these two types would be expected to appear in our backcross population. When our backcross population is classified according to the criteria just stated (see Table III), we find that we have obtained relatively equal numbers of pups bearing one Dalmatian spotting pattern gene and pups bearing two Dalmatian spotting pattern genes. Of seven pups having one Dalmatian spotting gene, two bear "low uric acid excretion" and five have "high uric acid excretion." Of eight bearing two genes for Dalmatian spotting pattern, six have "low uric acid excretion" and two have "high uric acid excretion." Since the "high uric acid excretion" character and the Dalmatian spotting pattern entered the original cross together, and since the "low uric acid excretion" and non-Dalmatian spotting character entered the original cross together, these same combinations should be most frequent in the backcross population if any linkage existed. Otherwise the four classes should be relatively equal in numbers. Curiously enough, the new combinations (crossover classes) of (a) a non-Dalmatian spotting pattern with "high uric acid excretion," and (b) a Dalmatian spotting pattern with "low uric acid excretion," actually predominate over the two classes made up of old combinations. This indicates that the Dalmatian spotting pattern character and the "high uric acid excretion" character are determined by genes in different chromosomes, because there is no evidence of linkage.

The two F_2 litters yielded data quite in accord with those of the backcross experiments. When the ten pups constituting the two F_2 litters are classified upon the same basis (see Table IV), we find three had two Dalmatian spotting pattern genes, six had one Dalmatian spotting pattern gene, and one had no Dalmatian spotting pattern gene. Only the latter animal had the "high uric acid excretion" character. The nine others were "low-producers" of uric acid. The presence of three pups with two genes for Dalmatian spotting pattern and "low uric acid excretion" indicates that a crossover must have taken place in one of the hybrid parents if a linkage exists. The presence of the pup ($F_2 7$) with no Dalmatian spotting pattern gene and "high uric acid excretion" would indicate two crossovers if linkage existed. Such a high percentage of detectable crossover combinations is quite in accordance with free segregation indicated by our backcross data.

To summarize our genetic findings, we may say that "high uric acid ex-

TABLE I. Uric Acid Excretion in F_1 Hybrids

Father	Mother	Offspring			
		Male		Female	
		Low uric acid excretion	High uric acid excretion	Low uric acid excretion	High uric acid excretion
$F_1 1$	$F_1 2$	2	0	4	0
$F_1 1$	$F_1 4$	1	0	2	1
$F_1 4$	$F_1 3$	2	1	3	0

Totals: Low uric acid exc. = 14; High uric acid exc. = 2

TABLE II. Uric Acid Excretion in F_2 Hybrids

Father	Mother	Offspring			
		Male		Female	
		Low uric acid excretion	High uric acid excretion	Low uric acid excretion	High uric acid excretion
$F_1 1$	$F_2 5$	1	1	1	1
$F_2 9$	$F_1 2$	1	1	2	0
$F_2 9$	$F_1 3$	1	0	1	2
$F_2 9$	$F_1 4$	1	0	0	2

Totals: Low uric acid exc. 8; High uric acid exc. 7.

TABLE III. Uric Acid Excretion and Spotting in F_2 Hybrids

One Dalmatian Spotting Gene		Two Dalmatian Spotting Genes	
Low uric acid excretion	High uric acid excretion	Low uric acid excretion	High uric acid excretion
Nos.	Nos.	Nos.	Nos.
3, 9	1, 2, 10, 13, 14	3, 4, 7, 8, 12, 15	6, 11

TABLE IV. Uric Acid Excretion and Spotting in F_2 Hybrids

No Dalmatian Spotting Gene		One Dalmatian Spotting Gene		Two Dalmatian Spotting Genes	
Low uric acid excretion	High uric acid excretion	Low uric acid excretion	High uric acid excretion	Low uric acid excretion	High uric acid excretion
	No. 7	Nos. 1, 2, 3, 4, 8, 9		Nos. 5, 6, 10	

cretion" is inherited as an almost completely recessive, non-sex-linked unit-character, dependent for its expression upon the presence of a single pair of Mendelizing genes. "High uric acid excretion" is in no way associated with the gene complex producing the harlequin type of spotting, in combination with which it is so frequently found in Dalmatian coach hounds. Our linkage tests indicate that the genes underlying "high uric acid excretion" and the genes underlying the production of Dalmatian spotting are resident in independent pairs of chromosomes.

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QUINTS' PROGRESS

Do They "Prove" Heredity or Environment?

THIS book* is divided into six sections: (1) A Biological Study, (2) The Mental Growth, (3) The Early Social Development, (4) The Development of Self Discipline, (5) Routine Training (sleeping, eating, elimination routine, washing, dressing and play), and (6) Early Development in Spoken Language of the Dionne Qpintuplets.

The first section deals with biological characters. It shows that in palm and finger print patterns the children are more alike than their sibs are alike, and are as alike as other sets of identical quadruplets and triplets studied. Their blood group was uniformly *O*, but as the groupings of the parents and other sibs are not known, the significance of this evidence of their being identical is problematical and can contribute little to the proof. The eye color is alike in the five children, while the sibs examined showed segregation of eye color, one of them being fair haired and light eyed. All have a refractive error, *Y*, *C* and *A* having an error of plus .75 in both eyes, *E* an error of plus .50 and *M* of plus 1.25. Both *E* and *M* exhibited strabismus, which is gradually disappearing in *E*. The hair is the same color in all; all five possess an epicanthal lid fold which some of the sibs also show. The hair whorl is counterclockwise in four, and clockwise in *M*. *E* is definitely left handed. All show a marked facial asymmetry, and, interestingly enough, of the same type. The left side of the face is long, narrow, oval with the mouth definitely lower at the outer corner and the lips thinner than on the right side of the face, which is shorter and much squarer in contour.

Causes of Retardation

In the chapter on mental development, it is pointed out that these children were two months premature, that premature children do not make up that handicap for some years, and that therefore these five are not to be compared with any normal standard. Therefore, no intelligence quotients are given. In motor behavior the five more nearly reach the norm than in adaptive behavior, personal-social behavior and language, which latter is the most retarded.

The conclusions reached in this chapter are that the *apparent* retardation may

*Collected Studies of the Dionne Quintuplets. Edited by W. E. Blatz. Pp. 238. Many plates and charts. $4.00. University of Toronto Press. 1937.

be accounted for by prematurity, environmental arrangements and inheritance. The environmental arrangements are supposed to have delayed the development of the children because they have no contacts or few with older children which would stimulate them to talk. The sometimes astounding vocabularies of only children reared under isolated conditions does not make this explanation very convincing, though the environment has certainly been unusual. Inheritance, the authors say, cannot be evaluated.

From observations on the social development of the children, it is concluded that "these five children already manifest quantitative and qualitative social and personality differences of a more or less stable nature," and that these differences are environmental rather than hereditary.

The impression gained from the book is that the psychologists are striving to show the greater effect of environment in differentiating these children into five distinct patterns. To the geneticist, who admits that environment plays a large rôle in this, the striking similarity of the curves of motor, adaptive, personal-social behavior and language development plotted by psychologists who certainly were not trying to prove the importance of an inherited background, is most significant. Thus the graphs show that in all five, whatever the relation of the motor and personal-social behavior curves was before 18 months, at that time, the latter development equalled or rose above the progress of motor development, and continued above it or equalled it in all cases from that time on; that language development in all was the lowest curve from 18 months on. Individual differences there certainly appear to be, and no doubt conditioned largely by physical development, hence the earlier acquisition of skills by the best developed of the five; but the similarity of progression of all five is interesting, when compared with the norm.

It will be most interesting to watch the further development of these children, who have had in constant physical care, in most carefully guarded environment, in constant adult guidance what very few other children ever have. Most children grow up while their mothers are washing dishes, ironing clothes, getting meals, and there may be not only no association with other children in the first few years, (as is the case with a first child), but no constant guidance by the adult mind to stimulate development. If proper environment will do what some of the psychologists claim, these children should be showing an I. Q. far in excess of normal, after they have had a chance to overcome their two months' prematurity. In the meantime, one wonders just what being "mentally delayed" but not "mentally retarded" means. Apparently something to the psychologists, if not to the rest of us! Also, how long does it take a two-months premature child to catch up in development? If environment plays such a rôle, certainly the post-natal environment is more stimulating than the intra-uterine. Although physically handicapped, mentally the premature child should have the advantage. Certainly, one cannot help marvelling that two months' prematurity should cause almost two years' delay in language development, for the vocabulary of these children at 38 months was not much more than many children possess at 12 to 14 months. Here is undoubtedly a great opportunity for those who hold "That the inheritance of mental traits has gone the way of all other long discarded superstitions" to prove their point about environment being able to mold anything out of the plastic clay of childhood. So far, with a vocabulary of only 80-odd words among the five of them at three years of age, and the acknowledgment of a mental "delay," although not a "retardation," environment seems to have been playing a losing game.

MADGE THURLOW MACKLIN.

STRUCTURAL HYBRIDITY IN *NOTHOSCORDUM FRAGRANS*

and the Origin of Terminally Attached Chromosomes

ALBERT LEVAN and S. L. EMSWELLER*

A FORM of *Nothoscordum fragrans* Kunth (=*Allium fragrans* Vent.) grown at the Bell Station, Maryland, was studied cytologically during a month's stay of the senior author at the United States Horticultural Station, Beltsville, Md. It originated from seeds obtained from the Cambridge Botanical Garden, England, and exhibited certain interesting features, which in our opinion justify the publishing of the following preliminary note.

Earlier studies on *Nothoscordum* species by Koerperich[4] reported the chromosome number for one *fragrans* form as $2n=16$. Later Levan,[5] working with another *fragrans* reported the number as $2n=18$. A closely related form, *bivalve*, was shown by Anderson[1] and Beal[2] to have a similar count, $2n=18$. In the *fragrans* form studied by Koerperich[4] all 16 chromosomes had median attachments, while in the 18 chromosome *fragrans* and *bivalve* forms there occurred four with terminal attachments. Since these terminally attached chromosomes were about the same length as one arm of the longest biarmed chromosomes present, their origin has been explained by a fragmentation of one long biarmed chromosome in the eight-chromosome *Nothoscordum* genom. This assumption is in agreement with the conditions of 7, 8, and 9 chromosome Alliums reported by Levan.[4] An autotetraploid *fragrans* with $2n=32$ chromosomes occurs and has been reported by Matsuura and Sutô.[7] The basic chromosome set consisted of seven with median and one with a subterminal attachment.

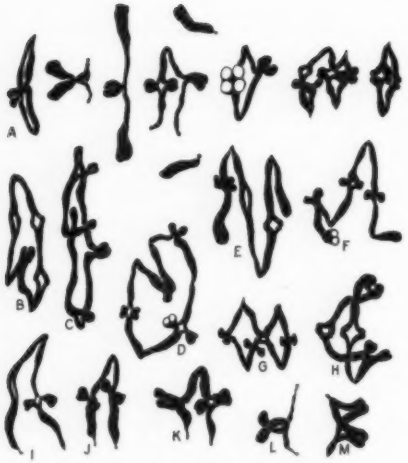
The *Nothoscordum* form studied by us differed from the other forms in having 19 somatic chromosomes. It was thus characterized by nuclear asymmetry. The appearance of the chromosomes is shown in Figures 5, 7, and 8. The two types of chromosomes were readily recognized; 13 having median attachment and six being terminal. Hereafter these two types will be referred to as *m* and *t*, respectively. The *t* chromosomes each had a little satellite proximally attached.

As already pointed out earlier by Levan,[5] the *Nothoscordum* chromosomes are very large, the longest *m* chromosomes, measured in the pollen division, having a length of 21 μ and the shortest about 12 μ. The *t* chromosomes measure 11-13 μ, which seems to be a little longer than one arm of the longest *m* chromosome.

Meiosis. — The prophases have not been studied. In the first metaphase, bivalents and higher chromosome complexes are formed. The situation most commonly found is the one diagrammatically shown in Figure 6. Here there is one quadrivalent formed by four *m* chromosomes, one asymmetric trivalent consisting of one *m* and two *t* chromosomes, four bivalents formed by *m* chromosomes and two by *t* chromosomes. There sometimes occur univalents, which in most cases were identified as *t* chromosomes (Figure 5*A*). Some few configurations indicated that very rarely higher complexes than four chromosomes were formed.

The bivalents have the appearance commonly found in Allium, — the *m*

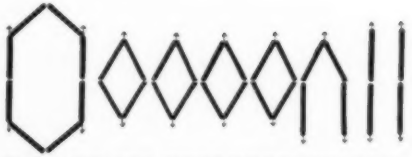
*Sugar Beet Breeding Station, Landskrona, Sweden, and Collaborator United States Department of Agriculture; and Division of Fruit and Vegetable Crops and Diseases, U. S. Department of Agriculture, respectively.



CHROMOSOMES IN MEIOTIC DIVISIONS

Figure 5

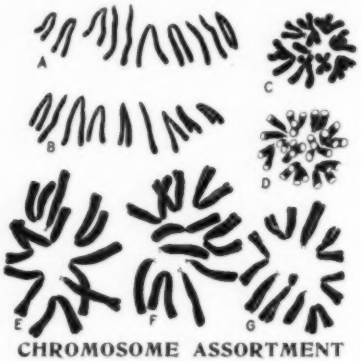
First and second division figures and microspore chromosomes (see text).



HOW UNLIKE CHROMOSOMES PAIR

Figure 6

Diagrammatic drawing of I metaphase. One quadrivalent of 4 *m*, 1 trivalent consisting of 1 *m* and 2 *t* chromosomes, 4 bivalents formed by *m* chromosomes, and 2 by *t* chromosomes.



CHROMOSOME ASSORTMENT

Figure 7

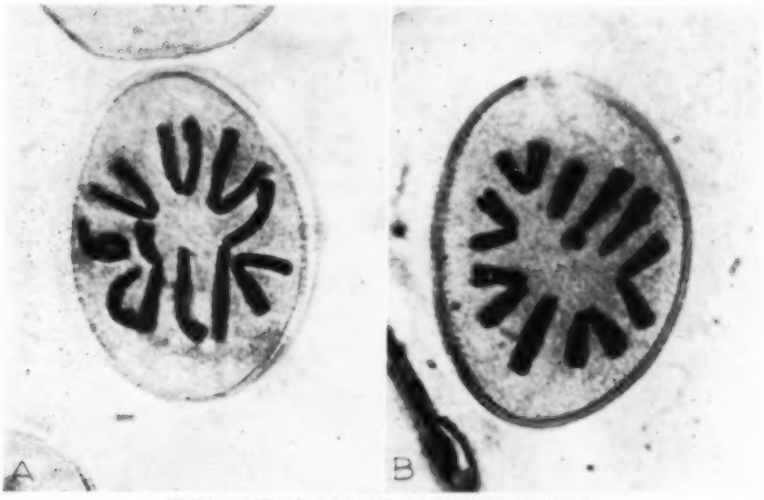
Typical configurations of I metaphase—bivalents, trivalents, and quadrivalents (see text).

chromosomes forming rings or rods, the *t* chromosomes forming rods. There often occurs more than one chiasma on each arm. This condition may occasionally cause the *t* chromosomes to form ring-shaped bivalents.

The complex of four chromosomes resembles the amphibivalent found earlier by Levan in *Allium ammophilum*.[5] It most often takes the configuration of a ring or chain of four (Figure 5*B-H*). The ring of four is often arranged in zig-zag (Figure 5*A, G*). Non-terminalized chiasmata give rise to the very characteristic cross-arms of the amphibivalent. The amphibivalents early in metaphase reach a balanced equilibrium position on the polar spindle and the anaphase separation takes place very regularly, two chromosomes being distributed to each pole. These very simple pairing conditions of the amphibivalent suggest that its origin is due to a single interchange which occurred very close to the attachment constrictions.

The heterotrivalent consists of one *m* and two *t* chromosomes (Figure 5*A, I-K*). The *t* chromosomes must be at least partly homologous with the *m* chromosomes, since one is paired distally with one arm of the *m*, and the second is similarly paired with the other arm. This condition supports very strongly the theory mentioned above concerning the possibility of the *t* chromosomes originating by fragmentation of *m* chromosomes.

The commonest trivalent type observed is a chain of three bent at the spindle attachment of the *m* chromosomes, the two end links of the chain consisting of the *t* chromosomes. In this type the *m* chromosome is oriented towards one pole and both the *t* chromosomes toward the other. Sometimes the chain occurs straightened out, all three chromosomes lying in one line. In that case one *m* and one *t* chromosome may move to one pole and one single *t* to the other, or the *m* to one pole and the two *t* chromosomes to the other.



TWO CHROMOSOME ARRANGEMENTS

Figure 8

Photomicrographs of two microspore divisions. In *B* a "new" type chromosome with sub-terminal insertion can be seen at one o'clock. Such chromosomes were seen in less than one per cent of the pollen grains studied.

The first anaphase and telophase are regular. Lagging chromosomes are seldom found outside the telophase nuclei. As a rule the anaphases show groups of nine or ten chromosomes, i.e., 7 *m*+ 2 *t* and 6 *m* + 4 *t*, or 7 *m* + 3 *t* and 6 *m* + 3 *t* (Figure 7*C-D*). The second division takes place with great regularity, and in the second anaphase the different chromosome forms may be easily identified (Figure 7*A-B*).

The first pollen division. — As could be expected from the course of meiosis, the pollen grains of this *Nothoscordum* form contain varying chromosome complements. The conditions found in 250 pollen cells from seven different buds are summarized in Table 1. Most of the pollen grains contain 9 or 10 chromosomes (Figures 7*E-F*, 8*A*), only 14 out of 250 having another number. The frequency of *m* and *t* types varies, from five to eight *m* and from zero to four *t* chromosomes may be present. Eleven different types of chromosome complements were observed in the pollen grains. Two hundred and thirty-one of the 250 pollen grains studied fall into one of four classes, namely, 6 *m* + 3 *t*, 6 *m* + 4 *t*, 7 *m* + 2 *t*, and 7 *m* + 3 *t*. The remaining 19 are divided into seven types and may be considered as abnormal, probably originating through non-disjunction or loss of chromosomes.

As already mentioned, the amphibivalent behaved regularly during the first anaphase, two chromosomes going to each pole. Since the bivalents always separate regularly, the main cause of the chromosome variation in the pollen must be due to the heterotrivalent. In the two possible orientations of the trivalent we get the chromosome distributions as shown in Table II. If the orientations 1 and 2 of this table occur equally frequently and chromosome elimination does not occur, the four classes—*a*, *b*, *c*, and *d*—ought to have the same frequency. The study of meiosis, however, showed a preponderance of configuration 1. In the trivalents there was often observed a lagging *t* chromosome, which was probably eliminated later. The *m* chromosome on the other hand was always included in the daughter nucleus. This situation increases the oc-

currence of classes *a* and *d*, and this is probably the cause of the higher frequency of pollen grains with 7 *m* + 2 *t* and 6 *m* + 3 *t*. Pollen grains with six and seven chromosomes, however, should have the same frequency, and, in fact, 113 grains were found with six and 118 with seven *m* chromosomes.

The great uniformity in morphology of the pollen chromosomes is of interest. In spite of the pairing abnormalities during meiosis, there occurred almost exclusively *m* and *t* chromosomes in the pollen. This differs from the situation in the F_1 hybrid between *Allium fistulosum* and *A. cepa*, where all sorts of "new" chromosome types were observed in the pollen grains by Levan,[6] and Emsweller and Jones.[3] The pairing in *Nothoscordum* takes place with fairly great regularity. The polar fibers of homologous chromosomes are oriented opposite each other, or at any rate so nearly opposite that very rarely could a chiasma be formed between them. The observed irregularities in meiotic pairing must be due to very few structural changes. Probably one is a segmental interchange between two *m* chromosomes at a point close to the polar fiber attachment, and the other a fragmentation of one *m* chromosome at the insertion region, giving rise to two *t* chromosomes, each with a functioning terminal polar fiber insertion. A similar division of the polar fiber region has been reported in maize by McClintock.[8]

A morphologically different chromosome type was observed in only three pollen grains among the 250 studied. In all three cases it was an asymmetric chromosome with subterminal constriction. It is shown in the photomicrograph of Figure 8*B*, and the same cell in the camera lucida drawing of Figure 7*G*. The three new chromosomes had similar proportions, the arms being about eight μ and one μ in length, respectively.

At this time we have not studied the embryology of the plants investigated. The nucellar polyembryony, several times reported from *Nothoscordum*, is a mechanism likely to preserve an unbalanced chromosome set, which under the conditions of sexual reproduction would have been lost.

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TABLE I. The frequency of different chromosome complements in the first pollen division of Nothoscordum fragrans.

Plant no.	5 *m* + 5 *t*	6 *m* + 4 *t*	6 *m* + 5 *t*	7 *m* + 3 *t*	7 *m* + 4 *t*	8 *m* + 2 *t*	8 *m* + 3 *t*	9 *m* + 2 *t*	5 *m* + 6 *t*	10 *m* + 2 *t*	8 *m* + 6 *t*	Total
1	-	-	-	5	-	31	9	-	-	1	1	47
2	-	-	10	17	2	14	5	1	-	-	1	50
3	-	4	21	4	3	6	7	1	1	1	-	50
4	-	3	22	6	-	14	8	-	-	-	-	53
5	-	1	9	2	-	-	2	-	-	-	-	14
6	-	-	-	4	-	4	1	-	-	-	-	9
7	1	-	4	9	-	12	3	-	-	-	-	29
Total	1	6	66	47	5	85	35	2	1	2	2	250

TABLE II. Chromosome complements of the pollen grains resulting from different orientation of the heterotrivalent.

Meiotic orientation	1: ∩		2: —○—	
Chromosome number	a: 7*m* + 2*t*	b: 6*m* + 4*t*	c: 7*m* + 7*t*	d: 6*m* + 7*t*
Number of cases observed	83	47	35	66
Total	130		101	

METHODS OF CORRECTING PEDIGREE DATA

Comparison of results obtained by the "Sibling Method," the "Proband Method" and the "Percentage Affected Method" in determining the mode of inheritance of a trait in human families*

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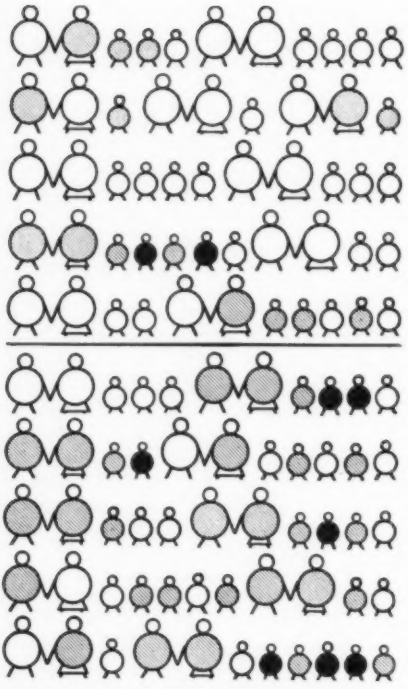
IT has been recognized for years by persons working in the field of human genetics, that a mathematical approach to the subject was necessary, especially since the method of experimental matings as a control to one's hypotheses was lacking. This was particularly necessary in those instances in which parents, unaffected by a certain trait, produced offspring who possessed it. The Mendelian theory provided an explanation for such a phenomenon by assuming that the trait was dependent upon recessive factors resident in the germ plasm of both parents in single quantity. On the average one-fourth of all the offspring of such parents would be expected to exhibit the recessive trait, and the other three-fourths would be normal. In human matings, where the families are small, this ratio of one affected to three normal is seldom attained. The number of affected children was usually more than one-fourth of the total number of offspring. It became necessary then to devise means to determine whether such a discrepancy in a given pedigree is caused by the small size of the human families, or whether it was caused by the trait in question not being dependent upon a single recessive factor.

The fact that more than one-fourth of the children are affected in pedigree data is readily explained when one considers that in some of the families in which the parents had a genetic constitution capable of producing an affected child, the affected child failed to appear. Such families would not be included in a series under investigation, and hence the absence of these normal children from the total number of normal children in the series would raise the percentage of affected offspring well above the theoretical 25 per cent demanded by the Mendelian theory. Take an example. If we consider 2-child families in which a recessive trait may be expected to appear in 25 per cent of the offspring, we find that the relative number of families having 1, 2 or no affected children in an average population is given by the expansion of the expression $(a + 3b)^2 = a^2 + 6ab + 9b^2$, where a indicates the chance of being affected, and $3b$ the chances of being normal. This means that on the average out of 16 couples with two children there will be 9 families who might have had an affected child, but who actually have only normal offspring. Six families will have one affected and one normal; and one family will have both children affected. In these seven families who have affected children there will be a total of 14 children with 8 affected, a percentage of 57.1 in place of the expected 25.

The "Sibling Method"

To correct this discrepancy, caused by the omission of the nine families where both children happened to be normal, Weinberg devised two methods. One was known as the "Geschwister" or "Sibling Method," and the other as the "Proband Method." In the first, the number of sibs of the affected children

*Read before the Royal Society of Canada, Toronto, on May 28, 1937.



RECESSIVE OUTCROPS IN HUMAN POPULATION

Figure 9

Recessive defects serious enough to be partiy self-limiting usually appear in human populations as sporadic outcrops when two normal "carriers" marry. This chart attempts to visualize how the "concentration" of a recessive gene in a population affects the frequency of such defects. The upper chart shows a group of families in which two-thirds of the parents are pure for the normal dominant gene; the lower chart a group in which only a fifth of the parents are not "carriers."

was determined, also the number of affected sibs of affected children, and the percentage of affected derived from that proportion. Thus the six affected children in the six families with only one affected would have six sibs, none of them affected. In the one family with two children affected, there would be a total of two sibs, for each affected child would have the other affected child as a sib, and there would be a total of two affected sibs. Hence there would be in these families a total of eight sibs of the affected children, and two of them, or 25 per cent, would be affected.

It will be seen, from Table I, that if the families in the series are not selected, but are examples of ideal random sampling, such as one gets from expansions of the expression $(a + 3b)$ raised to the power indicated by the number of children in the family, that the percentage of affected yielded by the sibling method is always 25, when the families having only normal children are omitted. This method then gives ideal percentages of affected when the data on which it is based are ideal, and departs from that as the families give evidence of being selected. It usually gives higher figures than 25 per cent, because of the tendency to report families where more than one are affected and to omit those where but one child shows the trait. For example, if a family of six children has but one affected with any trait it is not nearly so likely to be reported as if it had five or six affected. The greater the number of affected children, the more likely is the family to be reported.

The "Proband Method"

The proband method was therefore devised by Weinberg to correct this defect, and to eliminate as far as possible the weighting of the series in favor of too many affected. It accomplished this by removing from the series the affected person who brought the family manifesting the condition under study to the attention of the investigator. This person in genealogical tables is called the "proband," and hence this method is known as the "proband method." The number of normal and affected sibs of the proband are determined, and the percentage arrived at from those numbers. If all the other affected cases in the family are discovered through the investigation started by the proband, they are listed as secondary cases. If several of the affected members of a family came for treatment, each affected person is called a proband, and all the others affected in the family are listed as second-

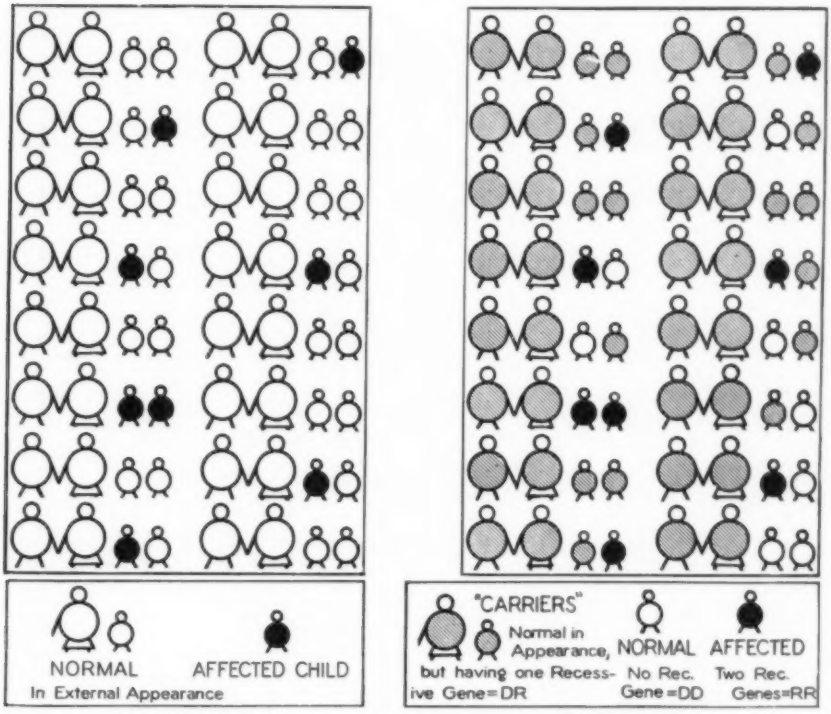


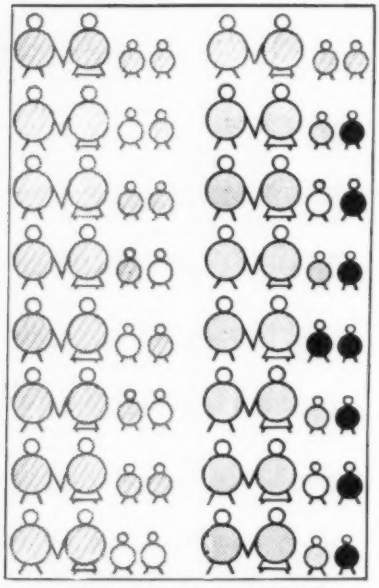
Figure 10

In all of the families both parents are carriers of a recessive gene. In only seven of them do affected children actually appear, though in any of them defective children might be born. In a human population we cannot know how many families in addition to the ones which actually show a recessive trait, might show it. Distribution $= (a + 3b)^2$, where a stands for defective and b stands for normal and hybrid combined.

The genetic constitution of the families shown on the left is here diagrammed. The distribution of "carriers" of "pure normals" and of "pure recessives" is expressed by the formula $(n + 2h + a)^2$, where n stands for wholly normal, h stands for hybrid, and a stands for defective.

ary cases of that proband.

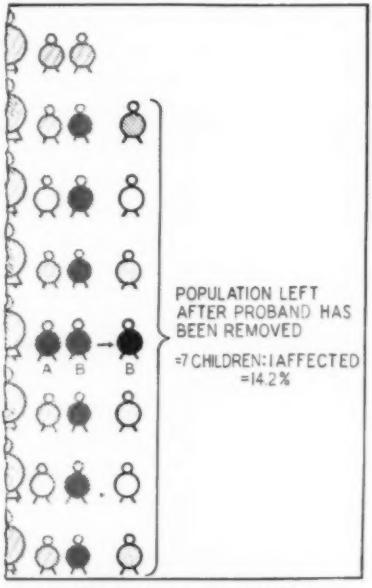
It becomes evident that some confusion will arise through determining who are to be counted as probands and who are to be regarded as purely secondary cases. If an affected person comes to one physician, and his affected sib to another physician, should the first physician, who may be the one investigating the family history, list the second affected sib as a proband or as a secondary case? It also becomes evident that the more frequently an affected child becomes a proband, and not a secondary case, the more nearly does the proband method become identical with the sibling method, and hence approach the perfect percentage of 25 to which the sibling method always attains in theoretical populations (Table I). The less often the affected children become probands, the less closely does the proband method approximate the ideal 25 per cent. If one is dealing with a condition in which the child dies with the disease shortly after birth, then the investigator who reports the family history must regard the earlier cases of the disease as secondary



THE SIBLING METHOD

Figure 11

The Sibling Method applied to an ideal situation, in sixteen two-child families. The nine families which do not have an affected child cannot be identified in the general population. Of the remaining families, one has two affected children; the others, one. In the Sibling Method only the siblings of affected individuals are counted. This gives a population of six normal and two affected (in the family with two affected, each affected individual is the sibling of the other), or 25 per cent,—the exact theoretical ratio.



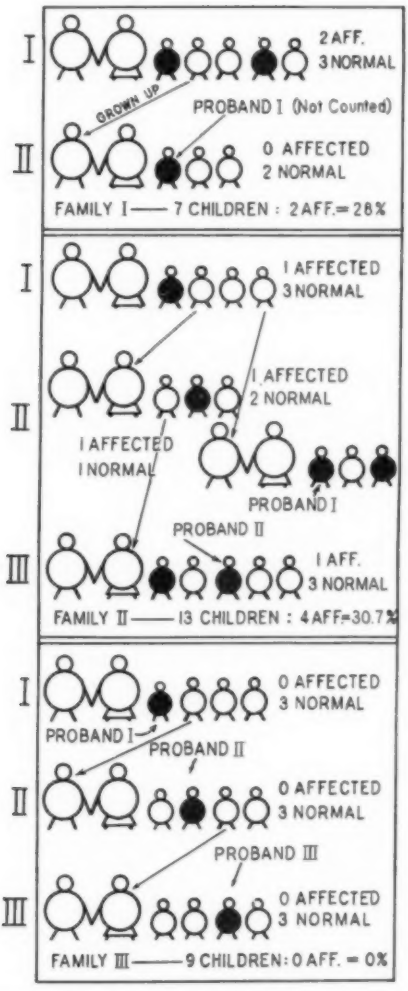
THE PROBAND METHOD

Figure 12

This chart shows the application of the "Proband Method" with the same ideal situation for two-child families charted for the "Sibling Method" at left. In families where there is but one affected individual, these are obviously all Probands, and are, therefore, *not counted* in compiling the totals. Only their normal sibs are counted. In the one family with two affected individuals (*A* and *B*), if one of these (*A*) comes to the attention of the investigator and the other is found as a result, then the Secondary Case (*B*)" is counted (= 14.2%). If both these affected individuals come to the attention of the investigator, then each one of them has an affected sib, and both affected individuals are counted. In that case the distribution is 2 : 8 or 25 per cent, exactly like the Sibling Method.

ones, not as probands, when he reports the family history. In such an instance, the proportion of affected children must of necessity fall below the theoretical expectation of 25 per cent, unless the population of affected families is a highly selected one, that is, selected because many in a family are affected.

Reference to Table II will show that such is the case. Here are populations of 2-, 3- and 4-child families showing random sampling, in which a recessive trait is appearing. The elimination of the affected persons from the group of affected in the large number of families in which only one child has the disease materially lowers the percentage of affected children. Thus in 2-child families there are seven families with eight affected children. If the one family with both affected had one of them a proband, and the other as a secondary case, then it leaves a total of seven children apart from the probands and a total of one affected apart from the probands. This gives a percentage of affected of 14.2, instead of 25. In 3-child families, there are 37 families who have affected chil-



THE PROBAND METHOD APPLIED TO PEDIGREES

Figure 13

Here are shown three examples of the Proband Method as it is applied in evaluating pedigree data. If an affected individual comes independently to the attention of the investigator he is a "Proband," the others are "secondary cases." In Family III, in three generations, each with four children and an ideal distribution of three normal to one defective, the three affected individuals all happen to be Probands. In that case, the distribution is nine normal: zero defective, or zero per cent affected instead of 25 per cent.

dren. Each family had a single proband. Therefore the number of children apart from the probands is 74, and the number of affected, which was 48, apart from the 37 probands is 11. The percentage of affected is therefore 14.8. In 6-child families the percentage has risen to 16.9; and in 13-child families to 19.4. Hence in populations in which we know the chances of being affected are one in four, and in which we know that the maximum of random sampling has been reached, we find that the proband method yields percentages of affected which are far too low. Either the full number of affected children must be regarded as probands, in which case this method becomes identical with the sibling method, and so is not needed, or the series of families must show a high degree of selectivity in having most of the families with more than one child affected.

For example, there are more families who have one child only affected, than have two or more affected in sibships up to and including the number five. Thus in 2-child families, there are six times as many with one affected as with more than one. In 3-child families the ratio is 27 : 10; in 4-child families it is 108 : 67; in 5-child families it is 405 : 376; and in 6-child families it is 1,458 : 1,909. Here then in 6-child families for the first time we get more families with two or more affected than have only one child with the condition. But the majority of families have fewer than six children; therefore the proband method which excludes the only affected member in families with but one affected, is highly inaccurate in the majority of families studied, when those families are representative of random sampling.

The binomial theorem which is the basis of the sibling and proband method can be used to determine the fit of the observed to the expected value with far less trouble than is necessary in either of the two methods of Weinberg, and moreover, it shows up clearly whether the material used is representative, or weighted in favor of families with more than one affected. Two diseases which are found in man have been selected as

examples and the data analyzed according to the sibling method, the proband method, and by the percentage method, giving the number of affected which would be expected on the basis of the binomial theorem. These diseases are amaurotic family iodiocy, and xeroderma pigmentosum. In none of the instances have the parents been affected so that if the diseases are dependent upon recessive factors, the matings are all of the $DR \times DR$ type, with one-quarter of the offspring liable to show the defect. In both diseases, the defect appears early in life, so that those who have inherited the defective genes usually live to show the disease, and therefore to be included in the series of affected. We will consider the data on amaurotic iodiocy first.

Analysis of Amaurotic Idiocy Families

This condition is one which appears at about six months of age and is fatal usually before the child is two years old. Before a second child can develop it in a family, the first child who had it is dead. Therefore when the family history is finally reported, the last affected child is the proband, and all others are secondary cases, which are dead at the time of investigation. I collected a series of families in which amaurotic idiocy had occurred. Table III gives the families with the total number of affected children and with the results of the sibling method, of the proband method, and of the percentage affected method. It will be noted that the final percentage of affected by the sibling method is 36.3 which is 11.3 per cent too high; while the proband method gives perfect agreement with the theoretical 25 per cent. The number of affected was actually 223 while the number expected by the percentage method was 186.3. If amaurotic idiocy is dependent upon a recessive gene, then it would appear that the proband method is ideal in giving reliable results. Paradoxically, the accuracy of our results are doubtful because on the other hand, if the condition is in fact due to a recessive gene, then the series of families here collected is not a random sampling, but holds far too many families with more than one child affected. For example, in the 2-child families, there were 22 families with one affected and 6 with two affected. As shown above, the ideal ratio is 6 : 1. Therefore this group includes nearly twice too many families with more than one affected. In the 3-child families, there are two families with all three children affected, seven with two affected, and 12 with one affected. From Table II, it will be seen that there should be approximately 18 families instead of seven in this group which had two children affected, and 54 instead of 12 which had but one affected if our distribution were proportional to the two families with all three children affected. Here, if amaurotic idiocy is due to recessive factors, there has been a definite tendency to exclude families where but one child was affected and to include families where more than one was affected.

In 4-child families, this is still more evident. There were two families with four children affected. Had these families been reported at random, there should have been approximately 216 4-child families with but one child affected, (there were actually five families); there should have been 108 4-child families with two affected, (there were actually two families); there should have been 24 families with three affected, (there were actually but two recorded). Again the families with one child affected were far too few in number in comparison to those reported with three and four children affected.

Thus the proband method, which appears to give ideal ratios of affected to normal if the disease is dependent upon a recessive factor, attains this result from a series, which on analysis proves to be not a random sampling of families with recessive traits. The high percentage of affected given by the sibling method from this series confirms this, for the percentage of affected by the sibling method approaches the ideal 25 as the series of families approaches the ideal of random sampling.

If amaurotic idiocy were due to a recurring dominant mutation, instead of recessive factors, it might appear in children whose parents were never affected, inasmuch as the affected person never lives long enough to become a parent. If the disease were a dominant character, then we should expect to find more families with two or more children affected than families with but one affected. Could it be that amaurotic idiocy is due to such a dominant mutation, and that the series of families we have here is illustrative of random sampling of such families? There are several facts which stand in the way of such an interpretation. The first is that in this series, about one-fifth of the families gave a history of relationship between the parents of the patients. In the case of dominant characters, their appearance is not increased by the marriage of cousins; but in the case of recessive characters, the frequency with which the character appears is increased by such inbreeding. Therefore the high rate of cousin marriages in this series would indicate that amaurotic idiocy is dependent upon recessive rather than dominant factors.

The second fact to be taken into consideration is that the series is no more representative of random sampling of families with a dominant character than it is of families with a recessive, perhaps even less so. It has been mentioned that there is always a tendency for the family with many affected to be reported while the family with but one affected is overlooked. If amaurotic idiocy were dependent upon a dominant mutation, we should have the curious anomaly of having too many families with but one child affected in contrast to those with more than one. Thus in 3-child families, where the disease is a dominant one, we should have an equal number of families with one child and with two children affected. In this series we have 12 families with one affected to seven with two affected. In 4-child families, there should be almost three times as many families with more than one affected as with only one, (11 : 4); but actually the numbers are about the same, (6 : 5). It may reasonably be argued therefore that amaurotic idiocy is not due to a dominant mutation, and that it may be dependent upon a recessive factor; and if so the series here collected has too many families with more than one affected. Finally it may be presented, that if the disease were due to a dominant mutation, the proband method is giving a percentage of 25 instead of the expected 50, for those affected. Therefore, the proband method when applied to actual series of cases gives figures that appear correct when the series is not a true random sample, and may be expected to give figures too low, when the series is a random sample.

Analysis of Xeroderma Pigmentosum Families

To illustrate this point, we will consider another disease, xeroderma pigmentosum. It also appears in children whose parents were normal and frequently related, and it appears early in life. I collected all the cases from the literature which was available to me, there being no selectivity exerted on my part in choosing cases in which more than one was affected. This set of data was examined, and the results will be given here (Table IV). Using the sibling method there were 26.4 per cent of the children affected, a very close agreement with the theoretical expectation of 25 per cent. By the proband method, only 17.3 per cent were affected, a rather wide departure from the expected 25 per cent. By the percentage method the number of affected expected if the defect were a recessive was 122, while actually 124 affected children were found. This is a very close agreement.

After having collected this series, I found a list of cases not available to me before reviewed by Cockayne, in which he had omitted all the families in which only one child was affected, and listed only those families in which more than one was affected, thus exerting a high degree of selectivity, and distinctly weighting the series in favor of too many affected on the basis of expectation. When this group was added to the orig-

inal series, the sibling method showed 27.1 per cent affected; the number expected affected was only 210.7, whereas the actual number of affected was 233. The proband method, however, was giving more perfect results on this series than on the first, the percentage now being 21.2. Thus, in a series, in which it is definitely known that families with only one affected were partially excluded, making it far from being a random sampling of affected families, the proband method gave better results than in a series which was as near random sampling as the original reporters had made it.

"Percentage Affected" Method

The "percentage affected" method used is one which recognizes that families in which the defect might have occurred but did not, have been excluded from the series. It gives opportunity to test whether the families in the series are a random sample, or whether they seem weighted in favor of those with too many affected; and it does not attempt to make the theoretical percentage of affected approach 25 by any artificial adjustment. It saves the trouble of deciding in a given family how many are to be considered as probands, and how many are secondary cases. It is easier of manipulation than the sibling method and requires less formulae to arrive at the result. It depends as do these other two methods upon the binomial theorem, and it calculates the percentage of affected which one would expect in a random sampling of children in 2-, 3- or 4-child families if the defect is a recessive, a dominant, dependent upon multiple factors, etc. Combining the determination of the probable error in the series, it makes allowance to some extent for those cases where the families have been reported because of the spectacular number of affected offpring produced.

The method is simple. The expression which shows the chances of being affected or normal, $(a + 3b)$ in the case of a recessive trait where both parents are hybrids, $(a + b)$ in the case of a dominant where one parent is a normal homozygote, the other an affected heterozygote, is expanded according to the number of children in the family. As seen from Table I, where a recessive trait occurs in 2-child families, there are seven families with a total of 14 children of whom eight or 57.1 per cent are affected. In 3-child families there are 37 families with a total of 111 children of whom 48 are affected, a percentage of 43.2. In 4-child families there are 175 families with a total of 700 children, of whom 256, or 36.5 per cent are affected.

If the defect in question is a dominant one, then in a series of 2-child families there would be 66.6 per cent affected in place of 50. Naturally when one comes to families with five, six or more children, one does not find enough families to make up the total expressed in the binomial theorem. One may nevertheless approximate to the correct percentage, since one is apt to find families with one, two or three affected in the proper ratios. For example, in 5-child families, theoretically there are 405 with one affected to every 270 with two affected to every 90 with three affected. Let us take just that group. That is a fair approximation of the ratio 5:3:1. We would find then that in our 5-child families this simpler ratio would hold, if the families were examples of random sampling. In these nine families with a total of 45 children, there would be 14 children affected, a percentage of 31. The theoretical percentage for 5-child families is 32.7. Here then with only nine families, in place of the 765 demanded by the theorum for 2, 3 and 4 affected, we get an almost ideal percentage. Yet the proband method with this same small group of families would have yielded but 14 per cent affected in place of 25, had there been but one proband per family.

Danger of Wrong Conclusions

This criticism is brought upon the proband method because it attains to the theoretical percentage only when all the affected children are probands, in which case it becomes identical with the sibling method, or when the series is abnormally weighted in favor of families

with more than one affected, in which case, the series is not an example of random sampling, and so loses most of its value for statistical analysis. The method is being used to some extent by psychiatrists to prove or disprove the inheritability and mode of inheritance of such diseases as dementia praecox. This is a condition which does not usually develop until early adult life; hence some of those who might develop it if it is an inherited condition, may have died before the disease developed. This would lessen the percentage of those affected below the figure expected. If the children are less than six in number, there will be more families with one only affected, than with two or more. Therefore to remove the proband from the family, and then determine the percentage of affected sibs among the children left, might easily give a figure of 0 percentage affected in a series of families in which there were five or fewer children reaching adulthood, even though the disease might be dependent upon a homozygous recessive gene. Before one can either accept or discard the hypothesis that dementia praecox is inherited as if dependent upon a single recessive gene substitution, the data will have to be reviewed and analysed by a method other than one which by its very nature is guaranteed to give erroneous results in families which are small and in which for the most part only one affected member will occur, and hence which will give apparently widely incorrect figures the more nearly ideal the series is as an expression of random sampling. It is not to be understood that I am arguing in favor of the idea that dementia praecox is dependent upon recessive factors, but merely that the proband method may not reveal that it is so dependent, even were this actually the case. All such data will have to be reanalysed by methods which give more reliable results.

TABLE I. Giving the theoretical percentage of affected children by the sibling method.

p	n	n(p - 1)	n(s - 1)	% affected
2	6	6	0	$a^2 + 6ab + 9b^2$
2	2	2	0	
3	27	54	19	$a^3 + 3a^2b + 27ab^2 + 27b^3$
3	18	36	18	
3	3	6	6	
4	108	324	0	
4	108	324	108	$a^4 + 12a^3b + 54a^2b^2 + 108ab^3$
4	36	108	72	$+ 81b^4$
4	4	12	12	
5	405	1620	0	
5	540	2160	540	
5	270	1080	540	$a^5 + 15a^4b + 90a^3b^2 + 270a^2b^3$
5	60	240	180	$+ 405ab^4 + 243b^5$
5	5	20	20	

p = number of children in the family.
n = number of recessives in the families indicated by formulae in right hand column.
n(p - 1) = number of siblings of recessives.
n(s - 1) = number of recessive sibs of recessives.

Expansion of binomial theorem in right hand column shows the number of families having two, two, three, etc. affected children in each size sibship. If the families with affected children are ideally distributed according to the binomial theorem, the sibling method gives the theoretical 25 per cent affected.

TABLE II. Percentage affected by Proband method in ideal theoretical population, with only one proband to a family.

Number of Families	Total Children	Total Children minus probands	Total affected minus probands	Percentage affected
$a^2 + 2ab$	14	7	1	14.2
$a^3 + 9a^2b + 27ab^2$	111	74	11	14.8
$a^4 + 12a^3b + 54a^2b^2 + 108ab^3$	700	525	81	15.4
$a^5 + 15a^4b + 90a^3b^2 + 270a^2b^3 + 405ab^4$	3905	3124	499	15.9
$a^6 + 18a^5b + 135a^4b^2 + 540a^3b^3 + 1215a^2b^4 + 1458ab^5$	20000	16835	2777	16.5

The percentage of affected in 7 child families apart from probants is 16.9; in 8 child families in 17.4; and has risen only to 19.4 in 13 child families. One proband might be expected in families in which the disease was one killing the child in early infancy, and so in which the physician, reporting the family, encounters the other affected members not as affected persons but as site of the patient.

TABLE III. Comparison of results obtained by three methods of analysis of amaurotic idiocy families

Size of family	Number of affected	Number of affected expected	Percentage affected sibling method	Percentage affected proband method
2	24	32	25.0	21.4
3	22	27.2	40.6	28.1
4	23	16.1	58	36.3
5	40	27.8	47.5	33.8
6	84	21.6	30	25
7	12	14.1	13.8	11.9
8	10	6.6	37.1	23.2
9	14	14.6	19.6	19.6
10	15	19.1	27.3	25
11	8	2.9	70	70
12	10	9.3	32.7	21.2
13	3	2.3	30.5	19.2
	255	184.3	38.3	25.1

The third column was derived from figuring the percentage of affected which would be found in a theoretical population of 2, 3, 4 etc. child families as found from the binomial theorem expanded in Table I. Thus 27.1% of the children of 2 child families should be affected, 42.2% of the 3 child families, etc. The number of affected actually found was too high because of the exclusion of families in which only one was affected. See Text.

TABLE IV. Comparison of results obtained by three methods of analysis of families with xeroderma pigmentosum from unselected cases.

Size of family	Number of affected	Number of affected expected	Percentage affected by sibling method	Percentage affected by proband method
2	15	14.0	66.6	18.4
3	14	14.0	66.6	18.4
4	23	21.9	28.0	29.2
5	13	14.3	19.4	V.S
6	17	17.7	34.9	23.1
7	9	10.1	10.5	13.3
8	10	11.1	30.0	14.9
9	3	3.4	20.0	25.0
10	4	5.5	10.6	11.1
11	0	0.0	10.0	10.0
12	0	0.1	10.1	10.1
13	7	3.5	20.0	20.0
14	0	0.0	0.0	0.0
	124	129.0	24.4	17.5

For explanation see Table II. Note here the almost perfect agreement between columns 2 and 3, and the close agreement between column 4 and the theoretical 25 per cent. Column 5 derived from the data furnished by the proband method gives a percentage much too low. Compare this with Table V, where the data are known to be selected in favor of families having more than one affected, and hence data which will give too high a percentage of affected.

TABLE V. Comparison of results obtained by three methods of analysis of families with xeroderma pigmentosum in which the families were selected in part because there was but one affected.

Size of family	Number of affected	Number of affected expected	Percentage affected by sibling method	Percentage affected by proband method
2	17	16.0	33.3	21.4
3	21	24.0	62.1	31.0
4	27	29.1	28.8	28.7
5	31	27.8	30.6	30.6
6	28	21.9	30.0	23.0
7	15	14.8	24.4	19.0
8	24	22.6	19.8	16.6
9	21	17.0	27.3	23.0
10	6	10.6	18.9	11.1
11	0	0.8	10.0	10.0
12	0	0.1	10.1	10.0
13	4	3.3	30.0	30.0
14	2	2.6	7.7	7.7
15	0	0.0	0.0	0.0
	255	210.7	27.1	21.2

Note that when the series of families is known to contain too many with more than one child affected, the results of the "percentage affected" method as given in column 5 are too low as would be expected; the sibling method is too high as would be expected, and the proband method comes nearer to the expected 25 per cent than in Table IV, where the data were examples of random sampling.

HALDANE LOOKS AT EUGENICS

A Review of *Heredity and Politics*

THE publishers claim that Haldane shows the reader "how this science of heredity applies to men, women and nations." Actually the author displays more zest in pointing out ways in which a pseudo-science of heredity has formed the basis of rationalizations and legislative acts designed to curb the reproduction of the depressed or oppressed groups in various societies. Although the examples are carefully chosen elsewhere, the analysis was apparently written with the "racial science" of contemporary Germany in mind.

The first section of the book outlines the principles of human heredity, as illustrated by certain hereditary abnormalities and the origin of hereditary diseases by mutation. This educational section for the non-technically trained person is admirably done. Haldane has succeeded in writing something other than a summary of the chapters of a Freshman college text; there are no diagrams of cells, detailed explanations of chromosomes and genes, or over-simplified theoretical diagrams of the transmission of traits. There is an excellent discussion of the possible types of interaction of nature and nurture.

The remainder of the volume discusses differential fertility, the probable future trend of national intelligence, the nature of racial differences, problems of race mixture, and the biological basis for negative eugenic measures. Unfortunately the methods so clearly outlined in the first portion of the volume are not applicable to the study of hereditary class differentials in intellect or personality. The type of detailed pedigree tracing the existence of haemophilia in the descendants of Queen Victoria would have little significance if the trait considered was the existence of political acumen in the Roosevelt family; the interpretation would be very difficult if it was schizophrenia. In other words, characteristics which, even if measurable quantitatively, represent a long-time process of interaction between heredity and environment cannot be studied by detailed analysis of individual pedigrees. Haldane gives little analysis of the method involved in the statistical study of the averages and variabilities of controlled samples. And this method alone can give a scientific basis for the analysis of the heredity of those human traits which vary more or less widely according to the environmental conditions in which they develop.

The second part of Haldane's volume, then, must be taken primarily as a statement of the personal views of the author and not as an authoritative statement of the position of biologists with reference to the rôle of heredity in group differentials in intelligence, or the social and biological validity of various reform measures. Let us take sterilization as an example, since its ridicule is Haldane's particular delight. He trains his largest guns on the extremists in the United States (aimed at Germany by a circuitous route), and blasts the views discarded by scientific biologists many years ago. It is admirably done, but one wonders if it may not be slightly extreme and perhaps even irrational. The proof of the misuse of a measure does not prove that it may not have some value.

Class and Race Differences

Haldane appears to be uncertain concerning the biological effects of differential fertility. He states emphatically that he does not believe that all the differences between social classes which are measured by the intelligence quotient are due to environment, but he doubts the truth of the deduction that the exist-

*HALDANE, J. B. S. Heredity and politics. 202 pp. New York, W. W. Norton and Co., 1938.

ing differential fertility of social classes must be producing a decline in the median intelligence of the population. His position with regard to race is very similar. "There is no question in my mind of the existence of racial differences in psychology, but I do not know to what they are due." He states, "I find it very hard to rule out nature," but he is agnostic with reference to the general superiority of any particular race.

Haldane's discussion of human inequality raises again the question of the influence of class bias in eugenic philosophies. Undoubtedly much eugenic propaganda has been based on an identification of superior social-economic status with superior innate ability, and vice versa. However, it is scarcely more scientific to start from "left" premises and assume that there are no significant differences in the averages or the distributions of mental traits as between different groups. Haldane's analysis throws into sharp relief the difficulties facing an organization attempting to secure eugenic reforms in a heterogeneous culture. Presumably such a group could secure fair agreement on what is desirable in negative eugenics; the disagreement would concern means rather than ends. But in positive eugenics the ends themselves are not matters of easy concensus. The determination of biological superiority is an evaluative process, difficult even in that future era when the science of human heredity can explain the rôle of inheritance in those things we call intelligence and personality. And the means utilized to secure the agreed ends would differ according to the organization and values of the dominant group in the society in question.

Finally, Haldane does not believe that any eugenic measures, even those which he approves, are likely to be of much importance; in his opinion, the fundamental causes determining the development of human society in the near future will be economic. He infers that the ideal for biologists would be to return to their laboratories and avoid becoming protagonists of political or social philosophies claiming biological validity—after he has completed his own exposition.

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WHY WE BEHAVE LIKE MEN AND WOMEN

THIS book is another solid bit of evidence in support of the conviction of the Stanford University group that mental traits can be analyzed and measured. The senior author, a pioneer in the measurement of intelligence, has now, once more, turned his own keen intellect to devise means for the experimental measurement of a more "subjective" mental trait of personality, namely sex-temperament. With Catherine Cox Miles, author of *Early Mental Traits of 300 Geniuses,* as collaborator, and with the aid of nine assistants, the analysis of mental masculinity and femininity has been attempted. To do this it was first necessary to devise a test of this area of human expression, a test called briefly the *"M-F Scale,"* which was administered to several thousand subjects and standardized in terms of what men and women think and do—how they accept their respective rôles in life.

With a workable scale established, an attempt was made to glimpse the causal factors which shape the development of the *M-F* complex, by observing the relation of the *M-F* score to age, education, occupation, achievement, interests, and intelligence on the one hand; and to sex, physique and tendencies to homosexuality on the other. Physiological factors: hormones, endocrines, etc., were not included in this study. The future studies of sexual inversion should be materially aided by the perfection of the

*TERMAN, L. M., and C. Cox MILES. Sex and Personality: Studies in Masculinity and Femininity. 600 pp. $4.50. New York, McGraw-Hill.

M-F test-instrument, as well as from the attention given by the Stanford workers to the inborn and environmental determiners which make some men behave "like women," and some women "like men," at least so far as sex-temperament and conduct is recognized in our particular culture pattern. Three chapters of case studies: homosexual males; masculine and feminine types of delinquent girls, and some miscellaneous cases whose overt behavior and interests are inconsistent with their *M-F* scores, serve to emphasize the value of the new scale in clinical diagnosis and to indicate also that there is "more beyond" for the ambitious researcher who wishes to settle the question of personality and sexual-social adjustments.

"Mean" Men and "Womenly" Women

The *M-F Scale* is made up in two equivalent forms: *A* and *B*, composed of about 450 items in each form; it is of the group-test, pencil-paper, questionnaire variety, requiring 40 to 50 minutes to give, and embodying seven subtests: word association, ink-blot association, information, emotional and ethical responses, interests, personalities and opinions, introvertive responses. A "masculine" response gets a plus 1 mark; a "feminine" response, a minus 1 mark. Both forms are reproduced in an appendix, together with percentile norms for various population groups. The range of raw scores for normal male adults is from about +200 to —100, with a mean score of 52.58, s.d. 42.93; for normal female adults, the range is from —60 to —219, Mean of —70.65, s.d. 47.51.

Thus the two sex groups overlap, some men and women sharing in a common area of scores from + 60 to —100. But the mean man (this ominous phrase being used only in its statistical implications) at +52 does not intrude into the strictly feminine area; the average woman at —70 scarcely enters the outer boundary which incloses the males. In spite of the fact that this is a 20th-Century study, women are still very womanly, and (was it because the standardization was made out where the West begins?) men are *men*. Indeed, in a final line-up of some major groups, the only subgroup within the male whole to receive a "minus" average score are the few men designated as inverts, i.e., passive homosexuals, average score: —19.75±43.50. Next up are artists, average score .26± 39.04, followed by 70- and 80-year-old men who get an average score of 3.2± 7.04, both on the plus side! The most manly women are the college athletes of high intelligence, average: —13.7 ± .22.26, followed by Ph.D's and M.D.'s, average: —34.5±48.53. Not so very masculine after all! As consolation for disappointed masculine protestors, it may be noted that the most feminine female group includes dressmakers and domestics, average score: —103.9 ± 37.79.

How do the sexes differ in developing this *M-F* complex? At adolescence, 11th-grade boys are at the peak of their masculinity, which becomes progressively subdued throughout the subsequent decades. The fullest masculine expression in girls awaits the sophomore year at college, and then subsides somewhat irregularly with age. From age 50 on, women continue sweetly feminine. On the whole, higher education for men is associated with more masculinity, as is also intelligence and delinquency; theological training goes with a tendency to drop to the feminine end of the scale-score. Among women, intellectual achievement draws women up to the men's level, while music keeps them feminine. But the greatest differentiating factor for both male and female groups is to be found in the occupations which they pursue. The traveled male and the scientific female are elevated in the *M* end of the scale, while the religious and domestic interests depress scores toward the *F* end. For women, an interest in mechanics, politics, sport and art, gives those who are masculine-inclined, their best media for expression.

The application of a specially constructed *I* test of the sex-temperament aspects of inversion—itself a clinical con-

tribution to aid in early detection of the homosexual—suggests that the *P.M.H.* group (passive male homosexuals) are of two kinds: one tending to the *M*, the other to the *F* type. More research is needed here, as also for the factors behind the development of true sexual inversion. *P.M.H.* are more feminine, especially in fear responses and active male homosexuals more masculine on the average than are regular army men.

The Sexes *Do* Differ

To sum up this extensive and significant study: The sexes do differ in sex-temperament,—in their interests, emotions, impulses, activities. The rôles played by nature and nurture in setting up these differences cannot be answered finally. The association of the *M-F* scores with age, occupation, interests, etc., has been cited; that with cross-parent fixation and deviation toward the opposite-sex norm should also be mentioned. But without evidence as to whether these factors select the deviate or whether the deviate is molded by them. Unlike the cultural anthropologists who have gone completely environmental, the Stanford scientists do not take a definite stand on the heredity-environment causation of sex-personality. Their restraint, however, permits them to point out that even in primitive cultures, individual differences from the sex norm do occur.

Present-day science owes much to Terman and his students who have given us our most valid test of intelligence, and our best insight in regard to the origin of individual differences in intelligence. And now we are once more indebted for a better understanding of sex-temperament. But there is so much more still to know about our human race, so many traits, attitudes and personality revelations which should be explored for their origins and their distribution throughout the population groups of this country. While Professor Terman and the "Stanford group" could well rest on their laurels, we hope they won't.

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RACE AND DISEASE

THIS volume* presents the results of thought and research that have accrued over many years, in the field of comparative racial physiology and pathology. The authors of the various sections have covered their topics briefly but comprehensively and have indicated the directions for future research.

It is said that Grofjahn was the first to introduce the principles of racial differences based upon social status and standards of living. This pioneer contended that the types of disease, occupational disorders, the duration of a given illness, and the influence of environmental factors differed among the various social groups. The authors of the present book emphasize the point of view of race physiology and pathology and offer observations to show that fundamental normal anatomical and physiological differences in races determine many of the pathological characteristics, and also that additional variations are due to age and sex differences. The fact that the age at which sexual maturity occurs is different in different races is of considerable importance in the development of certain disease processes.

City and Country Diseases

It has been emphasized in numerous special studies that several pathological states reflect the essential differences between the inhabitants of cities, towns, villages and rural districts. There are also distinctive features appearing in

*SCHOTTKY, JOHANNES, Editor. Rasse und Krankheit. Pp. 468 plus xv; fig. 78: J. F. Lehmanns, München, 1937.

disease types and their duration due to the time element, that is, over a long period of time some diseases have disappeared entirely, others have become attenuated and lost their importance, while still others have shown a notable increase in incidence. Other factors also enter the picture, for example, Sitsen has indicated that during the World War tuberculosis in Europe in general and in Germany in particular was similar in its frequency, severity and type to the manifestations of the disease in the Dutch Indies where the inadequate living conditions were held responsible. Studies in the natural history of diseases, their rise and decline, the fluctuation of their manifestations should also be added to the list of aspects from which the racial angle is to be evaluated. Moreover, natural selection plays a part in the distribution of diseases. It is possible that the shifts in classification and definition of diseases by those in the medical sciences are responsible for some of the misleading conceptions in etiology, and certainly a number of pathological conditions, particularly infectious diseases, may be under control by the applications of medical science in one part of the world, while they flourish in other parts. In other than the bacteria-determined group, little is known regarding the local influence of scientific medicine on diseases.

Race Pathology

The relationships between race and disease manifestations are important from the standpoints of heredity and constitution, and they often play a part in racial, tribal, or group decline and extinction. However, decreases in marriages, and birth rates, shifts in social relations and the influence of foreign habit and custom which create discord in the characteristic national adjustments of a people also aid in their downfall. Thus there are biological, social and medical aspects to be considered together in a problem of this type.

The comments and observations presented in the book cover a wide field of pathology. Following the editor's introductory section which gives the historical survey, geographic demarcations and methods of studying the race problems, there are sections on race physiology (Flossner) in which the systems of the body are separately considered; on metabolic and constitutional diseases (Unverricht); on infections (de Rudder) in which epidemics and their results are related to racial factors; on tropical disorders (Olpp); on skin ailments (Schubert) and on neurological conditions (Kilm). Racial differences in the frequency and types of syphilis and the constitutional reactions characteristic of the disease are well described by Beringer.

The racial factors in the expression of various forms of mental disorder (Schottky) including epilepsy, feeblemindedness, neuroses, psychopathic personality, drug addiction and suicide are reviewed and discussed at some length. There are also valuable sections on surgery, gynecology and obstetrics, eye and ear characteristics and diseases, nose and throat affections, malignant growths, with the hereditary and racial aspects brought into the foreground, written by outstanding specialists in the respective fields. The development of teeth and their disorders (Proell) constitutes an interesting chapter, and the book is completed by a section on the relationship of mixed races to disease processes (Wülker).

The work brings up to date and makes more readily available the comparatively large number of observations in a wide field of research. The liberal bibliographies at the end of each section and an index of authors as well as a subject index enhance the value of the book.

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THE GENETIC FORTY-NINERS

A Review of the Genetic Yearbooks

GENETIC research and genetic "extension" in the Department of Agriculture have a peculiar interest to members of this Association,—not alone on account of a somewhat interwoven history but because of the insight which this gives regarding the application of science to agriculture. The Department of of Agriculture, curiously enough, began as a part of the Patent Office. Just a century ago, the Commissioner of Patents received an appropriation of $1,000.00 a year from Congress "to collect and publish agricultural statistics, and for other agricultural purposes." This mandate no doubt originated in the recognition of the need for more exact knowledge in Agriculture. Many of the leaders in the early Republic were scientific farmers in the best sense of the word—Washington and Jefferson both made fundamental contributions to agricultural practice. A growing population and McCormick's reaper perhaps emphasized in 1838 the need for greater precision and efficiency in farm practice.

The mere collection and publication of agricultural statistics was not enough, and the next important step was taken by the State Governments, which yielded to a growing demand by farmers for more knowledge, by establishing Agricultural Experiment Stations. Two of these have recently celebrated their fiftieth aniversaries—New Jersey and Minnesota. The commemorative volumes published by these institutions leave no doubt that the demand for more knowledge originated with the dirt farmer. That it has been insistent and persistent, and that it will continue can hardly be questioned.

After numerous vicissitudes the Department emerged as a major governmental agency in 1889, when the first secretary of Agriculture took his place on the President's cabinet. The early years of this new Department's existence were a continuation of the early mandate "to collect agricultural statistics." The interpretation and application of these statistical and other agricultural matters was left to the states or to the individual farmer. Not until after 1900 did the Department begin to become the great research and administrative organization we know today. In a brief review it would be impossible to attempt to untangle cause and effect, but there can be no doubt that between 1897 and 1913 while James Wilson of Iowa was Secretary of Agriculture, the groundwork was laid for the development of one of the great research organizations of the world. Its growth was not due to a conviction on the part of statesmen and politicians that more science was necessary in American Agriculture, but because practical farmers realized that they must have more knowledge if they were to prosper. The genetic yearbooks, and the other genetic publications of the Department which will surely follow them, represent the continuation of these demands by the practitioner of the art of agriculture that he shall have the fruits of scientific research.

Genetic Association Beginnings

Our interest in this background also centers around the fact that the moving spirit in organizing the American Genetic Association was Assistant Secretary of Agriculture under Mr. Wilson, Professor Willet M. Hays of Minnesota. This Association had its beginning in 1903 when a committee appointed by the Association of Land Grant Colleges founded the American Breeders' Association, with James Wilson as President and with Professor Hayes as Executive Secretary. At the time of his retirement from pub-

lic life in 1913, Secretary Wilson addressed the membership, summarizing the accomplishments of the Association in the following words:

> This Association has clearly blocked out for itself a new field. It has already done much to give direction to research work relating to heredity and to breeding. Through its conferences, public addresses, and publications it has done much to give direction, interest and impulse to the work of introducing new plants and animals; creating new varieties, families, and breeds; and encouraging the wider distribution and use of the more efficient strains of plants and animals . . . It has given ideas to universities and colleges, leading them to the establishment of lectureships and chairs of genetics and eugenics . . . This has also brought together the scientist and the practical breeder; and in addition you have, in your membership, brought together many of the leading genetic scientists of the world.

The sense almost of family pride which the members of this Association may therefore feel in the Yearbooks under review is strengthened by what the present Secretary of Agriculture has to say about genetics in his introductions to the two volumes:

> The science of the quality of life as it passes from generation to generation is in many respects the greatest and youngest of all sciences . . . The Yearbook shows how much we know and also how much more we should know but do not as yet. True, the science of genetics is still young and growing. I trust that the day will come when humanity will take as great an interest in the creation of new forms of living things as it now takes in the creation of superior forms of machinery. In the long run superior life forms may prove to have a greater profit for mankind than machinery . . . I would wish these yearbooks to be looked on as pointing the way toward a field of activity that will accomplish much more in the future than has been accomplished in the past. Life is always changing because environment is always changing. There are always new types of diseases, new insect, pests, changes in soil fertility, changes in consumer demands. The work of the plant and animal breeders is directed to meeting these changes. It has only just begun. We have reached our present stage of development largely by rule-of-thumb methods; but discoveries not dreamed of a few years ago are being made, and they counsel greater boldness in experiment and promise closer control because they give us an increased understanding of the processes that go in the minute cells where life has its beginning . . . I think that more knowledge of how to cooperate with nature for our own good is the greatest need of the world today. Man's control of his own future may depend in the long run on whether his biological knowledge, which is constructive, can catch up with his knowledge of the physical sciences, which has taught him so much about how to destroy.

Genetic Gold

With Mendel's publication the first outpost of the genetic army had gone forward into the essentially unknown land of the experimental study of heredity, where it remained a Lost Battalion of the army of science for thirty years, until DeVries, Correns and Tshermak found that Mendel's advance guard had actually coordinated a large sector of "enemy territory" and the first "Big Push" of the genetic advance was under way.

Unfortunately, as the army of science struggles forward on its head, complications set in. The very speed of an accelerated advance creates as a by-product of intellectual activity a sort of secretory armor of publications like the shell of the tortoise or the callouses on a well-worked hand. This has gone on so rapidly that in a third of a century the genetic front has developed from a time when it was hidden quiveringly behind a single modest booklet, to its present commanding, not to say disconcerting, position, where it overlooks a great country from the imposing vantage-point of a veritable mountain-range of publications. These paper and ink Alps and Andes unquestionably contain much gold, but the problem is to find it among the slag, and to refine it into useful knowledge. In 1934 Secretary of Agriculture Henry Wallace appointed a "Committee on Superior Germplasm," consisted of about a dozen experts from the Department's staff, and one member from the Carnegie Institution of Washington to look for the "Gold in them thar hills." These Genetic forty-niners took pen and record card and questionnaire in hand

and have emerged from their Genetic mountaineering with a remarkable summary of the most useful methods and the most hopeful sources of genetic material available to American agriculture.*

In appointing the Committee on Superior Germ Plasm, Secretary Wallace had a vision; possibly a bit Utopian, but not lacking in practical possibilities. Even though we are not ready to put a fence and appropriate labels around every gene useful to American farmers, his commission to the 'gene prospectors" did give a rare opportunity to sum up what was known and to present it in a form which would be truly chart and compass of American agriculture. Considering the history of this Association, we may be excused in commenting "at last"—and "so what next?"—the latter less wise-cracky than it sounds. The job badly needed to be done, and we would be scarcely human if we did not comment that some of the groundwork had been laid in the JOURNAL.

The purpose of the Department of Agriculture is ultimately practical, when all is said and done, and it must show greater efficiency and higher yields on American farms. Pure utilitarianism in applied science is stupidly short-sighted and there is great need for a more general understanding of the methods and promises of genetics, in a world where specialized crops are necessary, both to avoid the host of diseases modern transportation has spread throughout the world, and to produce the standardized products demanded by modern industry and commerce. The farmer of the future must become genetic-minded to an increasing extent. Mere "how to prune trees" farmer's bulletins are not enough: Only government is in position to do certain parts of the job of synthesis and interpretation which must be done. That job will not be finished for a long time yet, but the 1936-37 Yearbooks are a milestone in the long trek of genetic enlightenment.

Contents

The "Genetic Yearbooks" represent no mean mutation in the field of governmental publications. Heretofore, to judge by appearances, the *Yearbook* traced a clear lineage back to that original mandate to "collect and publish agricultural statistics." With over five hundred Congressmen acting as completely non-profit distributors, the *Yearbook* became a sort of agricultural statistical "Monkey-Ward" catalogue for an America which until recently has been predominantly rural. It was less useful, to be sure, than the true fruit of the mail order tree, because of a dearth of evocative illustrations and a paper too tough and abrasive. Now the statistics have been relegated to a separate volume, and the *Yearbook* deals not with bare numerological bones of agriculture, but with living realities. The Secretary's reports of what the Department of Agriculture was doing in 1936 and 1937 cover briefly all its multitudinous activities, and the other nine-tenths of the two volumes deal with how genetics and the work of the practical breeders give us better crops and herds and gardens. Subsequent issues of the *Yearbook* are planned to deal in the same way with other basic problems—soils (1938), nutrition, etc.

The lay of the Land of the Genes is succinctly set forth in introductory articles explaining what has been discovered about heredity by genetic research and by microscopical study of the mechanism of cell division and reproduction. This is followed by a detailed summary of existing knowl-

*1. Yearbook of Agriculture 1936. The Secretary's Report to the President (pp. 1-118)—Better Plants and Animals: A Survey of Superior Germ Plasm; Edited by Gove Hambidge and E. N. Bressman for the Secretary's Committee on Genetics (pp. 119-1189). $1.25. Government Printing Office, Washington. 1936.

2. Yearbook of Agriculture for 1937. The Secretary's Report to the President (pp. 1-116). Better Plants and Animals II (pp. 117-1497). $2.00. Government Printing Office, Washington. 1937.

edge regarding the genetics of organisms used on American farms. These summaries are each prepared by a worker or by a group specializing in the various fields covered, and naturally the methods of treatment and presentation are varied. Most of the contributors emphasize the great contributions which genetics has made to the art of breeding, but a heterodox contribution introducing the animal breeding section of the 1936 *Yearbook* raises the question just how much of practical value a knowledge of gene-behavior can be in organisms as complicated as the livestock mammals. Here, where selection must be carried on, not for single genes, but for groups of from three to a dozen, or a hundred genes, with poorly defined linkage relations, mostly unknown, the methods used by successful practical breeders in the past may not be much changed by genetic discoveries.

A detailed review of such a wealth of material is obviously out of the question. The reviewer knows of no other source of information regarding the background and practical applications of genetics in any way comparable to these two volumes. It must be remembered that by no means all of the field of the genetic advance has been covered. Thus the humble fruit fly ("pomace fly" the more exact terminology of the Yearbook has it) only enters into the discussion rather casually because fruit fly breeding is one activity not practiced on American farms. Since this is the organism on which more genetic research has been performed than on all other living things combined, another volume of 1,500 pages might be needed to document all that has been learned about the way heredity functions to build a complicated adult fruit fly out of the tiny single cell which constitutes the fertilized Drosophila egg.

In the case of our principal crops, their history is sketched as it goes back for centuries, with early developments often shrouded in mystery. The whole story of some new crops can be given by the men (or man) who made them, as in the case of the development of the cultivated blueberry. This is told by the late Frederick Coville, who took the wild blueberry of our woods and swamps and made it into a prolific cultivated plant which today is the principal crop in several regions which offered no particular promise for "conventional" agriculture. Dr. Coville made many contributions to the science of botany, but this was his greatest contribution to American agriculture. Describing a new variety just come into bearing, with fruit as much as an inch in diameter and of excellent flavor, he closed his article with these words:

> Toward the end of the present fiscal year I terminate my connection with the Department of Agriculture after 49 years of botanical research. It was the custom of Linnaeus and other scientific men of his time, when a public address was given in Latin, to end the address with a Latin word which meant "I have spoken," "I have said what I had to say," or "I am through." Therefore, with orthographic apologies to the Southern half of the United States, I name this blueberry and end this paper with that Latin word—*Dixi*.

Just a few days before his death Dr. Coville submitted the manuscript to the editor of the Yearbook. Thus this summary of thirty very fruitful years of inventive endeavor brought to completion, and gave the final touch to one of the most remarkable contributions that a single individual has ever made to the art and science of plant breeding.

The books will doubtless be required reference for most practitioners, teachers and students of genetics because they are the most complete summaries of applied genetics extant. They are the only books on genetics which a great many people will ever see, on account of the wide distribution of these volumes by the Government Printing Office and by members of Congress.

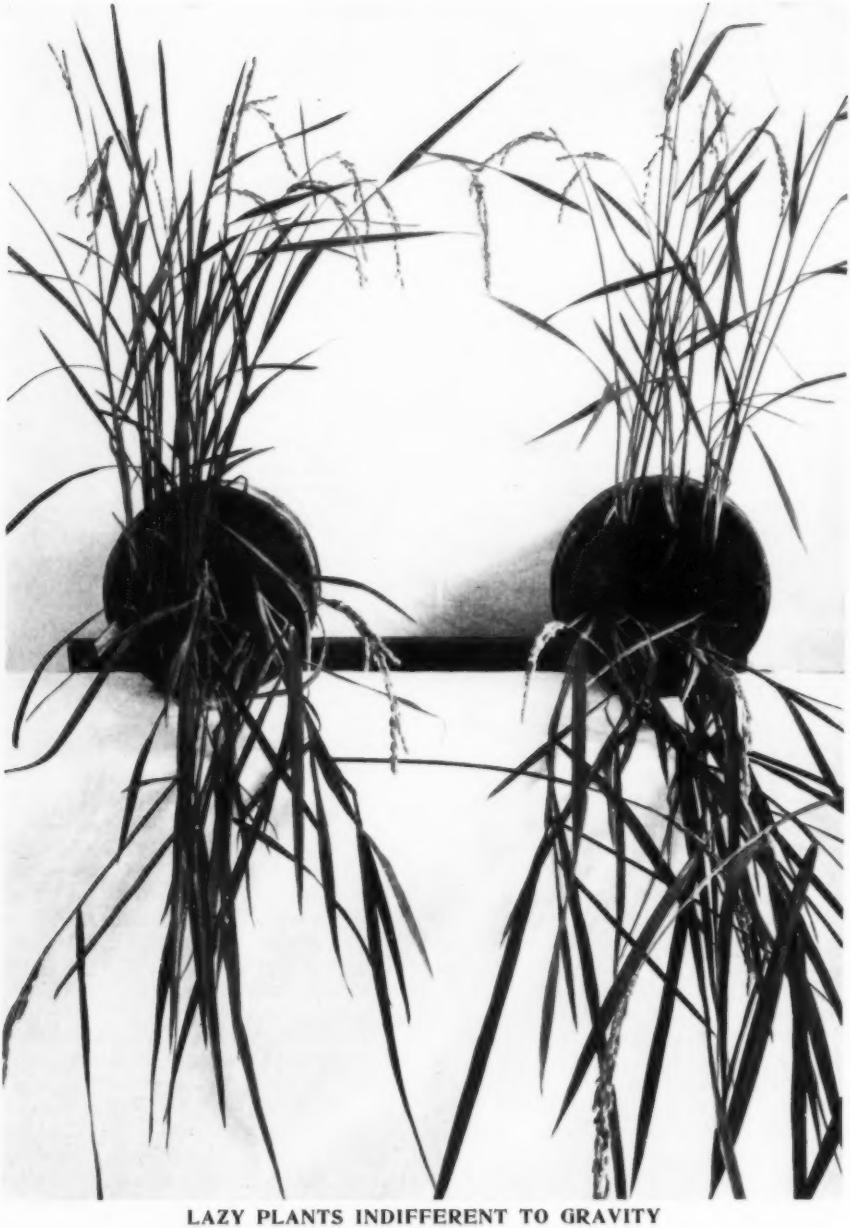
With the exciting ground-work of genetics thus covered quite completely at this time, it is to be hoped that supplements may be issued at not too

long intervals which will keep the story of genetic progress up-to-date without having to carry so great a weight of pre-1937 detail. There are indications that we may be on the verge of new techniques in compiling, interpreting and evaluating research. Recently the reformed British novelist, H. G. Wells, who has seen a Great Light regarding the future of mankind, addressed the Royal Society on the subject of a *World Encyclopaedia*. This monumental work was envisioned as indispensable to meet the great need of our time, not only *more knowledge* but a more general understanding of the knowledge we already have and an appreciation of its implications to human welfare. Wells' Encyclopaedia, its protagonist hopes, will be a source of intellectual and spiritual inspiration which will synthesize knowledge into wisdom, and utilize it constructively, where now undirected and undigested, it endangers the world in which we live. In one small branch of human knowledge the two Yearbooks might be considered as advance prospectuses of such a general survey and synthesis of what mankind knows about the universe of which he is a part.

The physical bulk and the intellectual complexity which make the volumes rather formidable in aspect emphasize dramatically the difficulties of interpretation and synthesis. The "men of good will" who it is hoped will somehow or other make over our rather inchoate 1938 world before it falls to pieces, must have infinite resources of intelligence and persistence if they are even going to be able to find out how this modern universe is put together. When it comes to "shattering this world to bits" as a cheerful preliminary to reconstructing it "nearer to our heart's desire"—that technique seems to promise even less success than came to the small boy whose curiosity as to the Christmas watch overcame his better judgment.

The volumes should help to give the research worker increasing appreciation and support from the general public, who in the end reaps the benefit. And be it remembered, tax-money expended on fundamental research is one form of governmental expenditure which pays dollars-and-cents dividends! Somehow we must finish the job which has been so well begun in telling Farmer Jones and John Q. Citizen just how important this *Science of Life* is, not only on the plane of fact and technology but as a sort of intellectual and emotional subfoundation to building any kind of planned society likely to *stay planned* by reason of fitting naturally into the genetic and cultural background of *Homo sapiens*.

It was said some years ago that genetics is progressing so rapidly that any textbook has begun to be out of date before it comes from the press. The same is true of the present publications, and since the last of them was issued, important new advances in inducing polyploidy in plants have been made independently by the non-governmental genetic forty-niner, A. F. Blakeslee, and by Nebel and Ruttle, on the staff of one of the State Experiment Stations. Thus the need is emphasized both for necessarily bulky and very expensive summaries (which these publications represent) as well as for the continuing news media such as this JOURNAL which record the month by month advance of the genetic front line—out in new fields when there is yet no "Literature Cited" and in which the existing knowledge is being synthesised into a truer picture of what life is and what it may become. In these untilled fields lies the future not only perhaps of American Agriculture but of Western civilization or of Human civilization as well, if Mr. Wallace is right in thinking that our real problem is to take the right side in the long battle between forces which build and those which blindly destroy.—R. C.



LAZY PLANTS INDIFFERENT TO GRAVITY

Figure 14

Normal and "lazy" plants in pots placed on their sides from March 16 to June 8, 1937. The normal plants were erect, while the "lazy" plants grew horizontally. "Lazy" rice differs from normal "upright" rice in a single genetic factor—"laziness" being recessive to normal growth habit.

A "LAZY" MUTATION IN RICE*

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A NUMBER of F_4 lines from the cross Caloro× Blue Rose (2914-A), at the Rice Branch Experiment Station, Stuttgart, Ark., segregated in 1933 for plants of normal and prostate growth habit. All such segregating lines were from the same F_2 family (2914A2). One selection from this family grown at the Rice Experiment Station, Crowley, La., in 1933 also segregated for normal and prostrate plants. In the single row, 41 plants were normal and 10 were prostrate, suggesting a 3:1 ratio.

The prostrate or "lazy" growth habit thus appears to be due to a recessive factor mutation that apparently occurred in an F_2 plant in 1931. All the F_3 progenies of this plant were of normal growth habit in 1932, but segregation for normal and prostrate growth habit occurred in the F_4 generation.

Description

"Lazy" seedlings grow upright, and for a time appear essentially normal, but as the plants become older and new tillers are formed, the main culms and the tillers assume a prostrate or in some cases a nearly prostrate position. There appears to be no regularity in the direction in which the plants lean. The culms of "lazy" plants transplanted to a greenhouse bed eventually became a tangled mass just above the surface of the water, extending in all directions. At or just before heading the culms sometimes bent up or sidewise at the upper or next to the upper nodes but with no apparent uniformity in direction. The culms of "lazy" plants are rather thick and appear to be fully as stiff as those of normal plants. The "lazy" plants are partly sterile, hence do not produce panicles that are sufficiently heavy when mature to induce lodging. Seeds of the "lazy" selection (2914A2-1) are similar to those of the Blue Rose parent. Normal and "lazy" plants are shown in the accompanying illustrations.

Inheritance of "Lazy" Habit

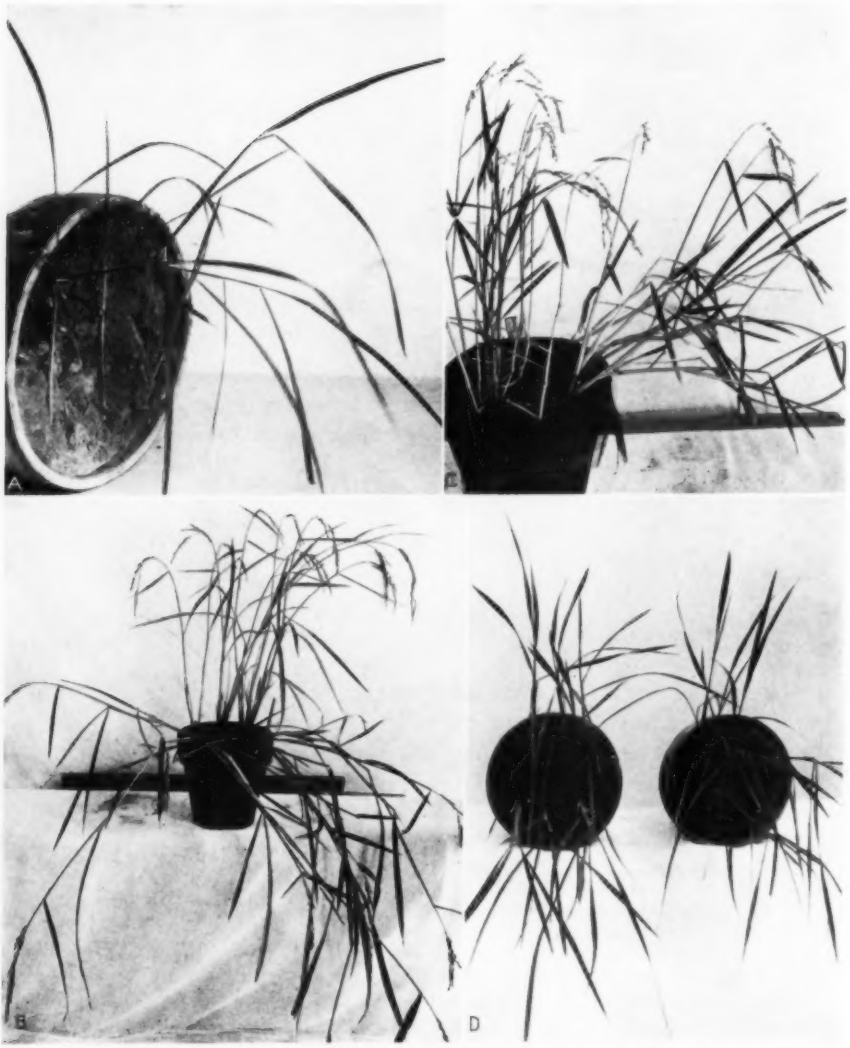
Seed from all plants in the ten segregating lines occurring at Stuttgart, Ark., in 1934 was sown in progeny rows in 1935. Of the 243 lines grown 59, containing 2,051 plants, bred true for normal growth habit; 118, containing 4,251 plants, segregated for normal and "lazy" habit; and 66, containing 2,136 plants, bred true for the "lazy" habit. These numbers agree with a 1:2:1 ratio of normal, segregating, and "lazy" lines. Eight of the ten segregating families gave normal and "lazy" plants in numbers that agreed with a 3:1 ratio, but in two families there was a rather wide deviation from a 3:1 ratio because of an excess of "lazy" plants. All plants classed as "lazy" in 1934 bred true for this character in 1935.

A cross between the "lazy" selection (2914A2-1) and Blue Rose (normal) was made in 1934. The F_1 plants, grown in a greenhouse, were nearly as erect as the Blue Rose parent and were partly sterile.

The F_2 population, grown in a greenhouse bed in 1936, consisted of 349 plants of which 273 were normal and 76 "lazy." These numbers also agree with a simple 3:1 ratio, and this segregation

*The writers express their appreciation to John W. Taylor, Agronomist, Arlington Experiment Farm, for assistance in determining the geotropic response of the plants.

†Senior Agronomist and Assistant Agronomist, respectively, Division of Cereal Crops and Diseases. Cooperative Investigations of the Division of Cereal Crops and Diseases, Bureau of Plant Industry, U. S. Dept. of Agriculture, and the Arkansas Agricultural Experiment Station.



RESPONSES OF NORMAL AND "LAZY" RICE PLANTS

Figure 15

A—Normal Caloro and "lazy" seedlings (2914A2-1) grown at an angle of about 90° from January 27 to March 4, 1937. Normal seedlings curved upward to an almost vertical position, right across the face of the pot, whereas "lazy" seedlings remained in the same position as when the pot was tilted. *B*—Normal and "lazy" plants grown from March 4 to June 9, 1937; normal plants erect, "lazy" plants nearly prostrate. *C*—Normal and "lazy" plants grown from March 16 to April 16 in a pot kept at an angle of 180° and upright from April 16 to June 9, 1937. Note the abrupt upright curve of the older culms of normal plants at the nodes. The "lazy" plants showed no such response and if unsupported by the rim of the pot would have been practically prostrate. *D*—Seedlings in pots tilted at an angle of 90° for 30 days. Normal seedlings assumed an erect position, but the "lazy" seedlings showed no marked gravitational response.

NORMAL PLANTS RIGHT THEMSELVES

Figure 16

Normal and "lazy" plants 30 days after the pot was inverted. The normal seedlings turned upward against the rim of the pot, while the "lazy" plants showed no marked geotropic response.

was confirmed in F_3 lines grown in 1937.

Seed from a partly sterile F_1 plant of the cross C. I. No. 2349 × Blue Rose was sent from Washington, D. C., to the Biggs Rice Field Station, Biggs, Calif., in the spring of 1936 and sown in rows. The F_2 population segregated, 82 plants being normal and 18 "lazy,"* again indicating a 3:1 ratio. Since both parental varieties were of normal erect growth habit, the appearance of "lazy" plants in this F_2 population must have been due to a recessive factor mutation. Blue Rose was used as the male parent in both crosses from which "lazy" plants were obtained.

Geotropic Response

The geotropic response of Caloro, Blue Rose, and the "lazy" selection (2914A2-1) was studied in the spring of 1937.

Seed of Caloro, Blue Rose, and the "lazy" selection were germinated between blotters at a temperature of about 30° C. The normal varieties produced vigorous seedlings, growing upright, and within two weeks they were lifting the blotting paper. The seedlings of the "lazy" strain also were vigorous but less erect than those of the normal seedlings and tended to grow horizontally rather than erect.

On January 27 normal and "lazy" seedlings, then two weeks old, were transplanted to soil in 8-inch clay pots. Three each of normal and "lazy" seedlings were placed in each pot. After transplanting, some of the pots were kept upright and others were tilted at angles of about 45° and 90°.

"Lazy" seedlings continued to grow in the same direction in which they were placed regardless of the position of the pots, whereas the normal seedlings in the tilted pots showed negative geotropism and were essentially perpendicular at the end of a 36-day period. The difference in response of the two types of seedlings is shown in Figure 15*A*.

The pot shown in Figure 15*A* was turned upright on March 4 and one week later the normal seedlings had assumed

*This material was grown by Loren L. Davis, Superintendent, Biggs Rice Field Station, Biggs, Calif.

an almost erect position, but the "lazy" seedlings showed no definite geotropic response.

Upright pots in which normal and "lazy" seedlings had been growing since January 27 were laid down or inverted on March 16 and a month later the culms of the normal plants were practically vertical, but the "lazy" culms remained in essentially the same position as they were when the pots were laid down (Figure 15*D*).

The culms of the normal plants in the inverted pots, at the end of 30 days, turned up to a partly vertical position, while the "lazy" culms still pointed downward (Figure 16).

Normal and "lazy" plants grown from March 16 to June 9 in pots placed on their sides are shown in Figure 14. The normal plants were erect but the "lazy" plants continued to grow horizontally.

The inverted pot shown in Figure 16 was turned upright on April 16 and again photographed on June 9 (Figure 15*C*). The older culms of the normal plants bent at the nodes toward an upright position, but the "lazy" plants showed no distinct geotropic response. The "lazy" plants are partly supported by the rim of the pot.

The Blue Rose seedlings and plants showed the same geotropic response as those of Caloro in all tests. The behavior of normal and "lazy" plants grown in different positions indicates that normal plants are negatively geotropic and that "lazy" plants are indifferent to gravity. There also may be morphological and histological differences between the two plant types, but the failure of the "lazy" seedlings to show the usual negative geotropic response appears to account for its abnormal growth habit.

"Lazy" selection 2914A2-1 apparently is gravitationally indifferent (ageotropic) and reacts the same as the "lazy" form of maize reported by Overbeek,* and an ageotropic mutation in X-rayed rice reported by Ramiah and Parthasarathi.† They, however, reported that in their rice ageotropic habit is dominant to normal growth habit, whereas "lazy" selection 2914A2-1 is a simple Mendelian recessive.

The dry matter content in ten plants 50 days old was 16.03 per cent in Caloro, 14.62 per cent in "lazy" plants, and 14.85 per cent in Blue Rose plants. The dry matter content in fully headed plants 102 days old was 25.93 per cent in Caloro plants, 27.18 per cent in "lazy" plants, and 27.04 per cent in Blue Rose plants. These figures indicate that "lazy" plants are not deficient in dry matter at either stage of growth.

Summary

The "lazy" habit of growth of selection 2914A2-1 appears to be due to a recessive factor mutation which probably occurred in an F_2 plant in 1931.

Genetic studies of the progeny from 10 segregating F_5 families from the same F_2 plant (2914A2) and F_2 and F_3 populations from the cross between "lazy" selection (2914A2-1) and Blue Rose indicate that the "lazy" growth habit is recessive to normal and differs from the normal by a single recessive factor.

Studies on the geotropic response of normal and "lazy" seedlings and plants indicate that the "lazy" plants are largely ageotropic while the normal plants are negatively geotropic.

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*Overbeek, J. van. "Lazy," an a-geotropic form of maize. *Jour. Hered.* 27:93-96. 1936.

†Ramiah, K., and Parthasarathi, N. An ageotropric mutation in x-rayed rice. *Current Sci.* 5:135-136. Sept., 1936.

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The Cover Illustration

The photograph shows a cluster of blueberries from plant GM37, which produced fruit measuring over an inch in diameter (25.9 mm.)—the largest blueberries ever produced. Since the fruit lacked the ultimate in flavor it was not introduced as a named variety. This plant traces back to four grandparents which were wild plants selected as of outstanding merit. It is one of the parents of the Dixie variety—which has berries nearly as large and of delicious flavor. The Dixie was the last variety named by Dr. F. V. Coville. For details see page 573-4 of the 1937 Yearbook.

A SKULL-DEFECT IN CATTLE

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SKULL DEFECT PROBABLY HEREDITARY

Figure 17

A—Skull of first calf showing the opening, which involves the frontal and parietal bones. *B*—Skull of second calf showing the defect. She is a full sister of the other affected calf. Anatomically this defect would be classified as a Meningoencephalocele and Proencephalus of Cranioschisis.

A HOLSTEIN-FRIESIAN calf with an unusual skull defect was born in the Idaho Agricultural Experiment Station herd on March 31, 1936. Since anatomical and physiological development during embryonic life give rise to various types of anomolies in our domestic animals, this skull defect was thought to be more satisfactorily explained on the basis of abnormal embryologic development than on a hereditary basis. On May 17, 1937, however,

*Published with the approval of the Director of the Idaho Agricultural Experiment Station as Research Paper No. 165.

BRAIN HERNIA DUE TO DEFECT
Figure 18

Head of first calf showing a part of the brain tissue protruding. The arrow points to the exposed brain tissue.

a full sister of the above-mentioned calf was born. This calf had the same skull deformity but was dead at birth, while the first calf died a few hours after birth. Since the mother of these calves had previously given birth to a normal calf from a different sire, the facts seemed to indicate that this skull defect might be hereditary.

Figure 18 shows the head of the first calf. The arrow points to the defect. The brain tissue protruding can easily be seen. The head of the second calf was quite similar except that more brain tissue was on the outside. In both cases the pia mater and dura mater membranes were missing.

Figure 17*A* shows the skull of the first calf. Both the frontal and parietal bones are affected. The skull was slightly damaged during the cleaning process as is noticeable.

Figure 17*B* shows the skull of the second calf. A much larger opening was present in this skull, and it affected both the frontal and parietal bones.

The defect illustrated in the accompanying photographs is similar, if not identical, to the skull defect in swine described by Nordby,[4,5] Hughes and Hart,[2] and similar to the skull defect in humans reported by Goldsmith.[1] It is apparently the same defect that Williams[6] has reported as "Hernia Cerebri." Keeler[3] has described a similar defect in mice. He calls the defect in mice "Parted Frontals" and points out that it is apparently inherited as a dominant unit character. Nordby[4] made close matings of swine known to carry the heritage necessary for producing the defect, but all the pigs farrowed were apparently physically normal. The litters produced varied in size from a half to two-thirds the usual number, indicating a lethal disturbance and resorbtions in early prenatal life. Keeler[3] found that the offspring of two affected parents are not all affected, and that the offspring of an affected parent and a normal unrelated strain not bearing "Parted Frontals" may be all affected, some affected, and none affected. Should the defect illustrated in the accompanying photographs be inherited in the same way as it is in mice, future mating of related animals may lead to a knowledge of the manner in which this defect is inherited. Should this defect often be lethal, with resorbtion in early pre-natal life, as suggested by Nordby,[4] an embryological study would be necessary in order to reveal information as to its heredity.

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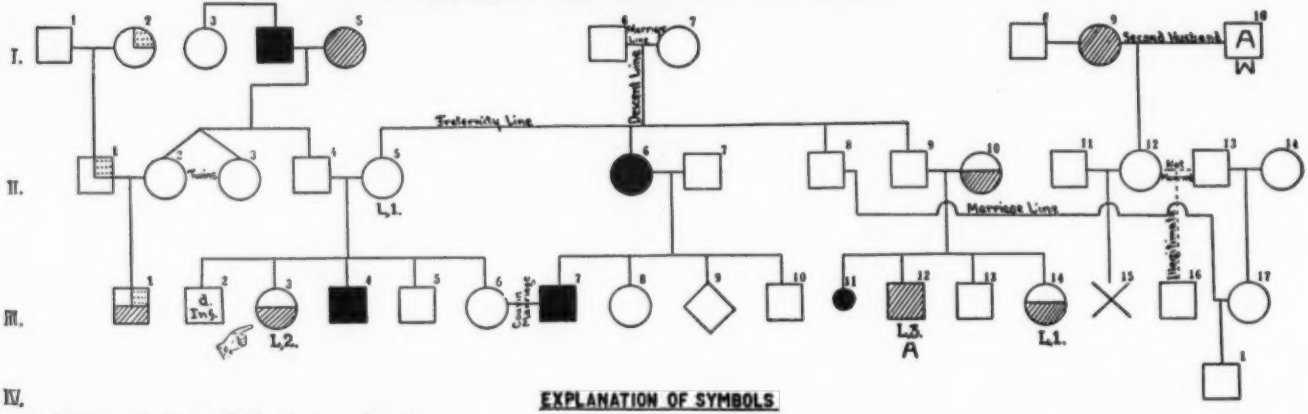
SAMPLE PEDIGREE CHART SHOWING THE MANNER OF CONSTRUCTION, AND THE USE OF STANDARD AND SPECIAL SYMBOLS.



EXPLANATION OF SYMBOLS

(a) Standard for all Pedigree Charts:

□=Male; ○=Female; ◇=Sex unknown; ●=Still-birth or Miscarriage; X=Children — number and sex unknown; ○○=Twins; d. Inf.=Died in Infancy; ☞ Points to the Propositus or central figure in the pedigree.

□—○=Parents.

□○□○○=Children.

Roman figures to the left indicate generations; Arabic figures locate individuals, (thus III, 7 is the young man in the third generation who married his cousin).

The following letters, placed in or around the individual's pedigree symbol, are standard for certain traits: A, alcoholic; B, blind; D, deaf; E, epileptic; F, feebleminded; I, insane; M, migraineous; N, normal in reference to traits under consideration; Ne, neurotic; P, paralytic; Sx, sexually immoral; S, syphilelic; T, tuberculous; W, wanderer.

(b) Especially devised or selected for this particular pedigree-chart:

■ ● = Successful leaders in politics. ◘ ◔ = Extra thumb on right hand. L.1.=Highly successful author. L.3.=Little or no ability in literary efforts. ▨ ◍ = Superior in vocal music. ▨ ◍ = Medium attainment in vocal music.

To fit the particular family and traits (whether physical, mental or temperamental; good or bad) under consideration, invent special symbols, or select special letters (in addition to those standardized under (a) above) to be placed within or near the particular individual's pedigree symbol, to indicate particular traits and their degree of development.

Prepared by the International Commission of Eugenics. See *Journal of Heredity* for August 1926. Additional copies may be obtained from A.G.A.

Material for Demonstrating Salivary Gland Chromosomes

Interrelated demonstration material—microscopic slides, chromosome maps, lantern slides and literature—make giant chromosomes available for classroom use.

MICROSCOPIC SLIDES OF SALIVARY GLAND CHROMOSOMES

Slide of Normal Salivary Chromosomes of *D. melanogaster*, each	$2.25
Three Slides	5.00
Slide with drawing identifying all chromosomes in two figures (Female)	4.00*
Slide showing Y-chromosome (Male)	4.00
Slide showing synapsis of normal and inverted X-chromosome (loop)	4.00*
Slide showing figure of an autosomal inversion	4.00*
Slide showing synapsis of translocated and normal chromosomes	4.00*
Slide showing a small deficiency (deletion)	4.00*
Salivary chromosomes of *D. virilis*	2.25
Three Slides	5.00
Normal Somatic (ganglion) chromosomes of *D. melanogaster* (*Magnification of at least 1000 × necessary to view satisfactorily.*)	**2.50**
1 Slide of each of above (9 slides—6 drawings)	25.00

* Three or more slides, 1 of each kind @ $3.50 each. Three or more of same kind @ $3.00 each.

SALIVARY GLAND CHROMOSOME MAPS

Painter's cytogenetic map of the salivary chromosomes, 9½ by 18 inches, line-cut showing major chromosome details and the genes approximately located to end of 1934, mailed unfolded	$ .50
Bridges' reference map of the banding of the salivary chromosomes, 9½ by 25 inches, halftone on heavy coated paper, unfolded	1.00
Bridges' Revised Map of the X-Chromosome—9½" x 18", unfolded	.75
Folded copies of Bridges' map, on lighter paper	.35
Hughes' map of Salivary Chromosomes of *Drosophila Virilis* (9½ by 18)	.75
1 copy each of Painter's, Hughes', and Bridges' two maps	2.50

LANTERN SLIDES OF SALIVARY CHROMOSOME MATERIAL

†21-323.	Salivary chromosomes in the Nucleus	$ .75
25-464.	"Portrait" of a Salivary Gland Chromosome	.75
25-466.	Giant Chromosomes Compared with "Normal", showing relative sizes	.75
25-469.	Synapsis of Normal and Inverted Chromosomes	.75
25-470.	Synapsis of Normal and Deleted Chromosomes	.75
25-472.	Painter's Cyto-Genetic Map of Salivary Chromosomes (Insert)	.75
26-62.	Salivary Chromosomes and Gonial Chromosomes Compared	.75
26-62.	Bridges' Reference Map of the Salivary Chromosomes of Drosophila	.75
26-176.	Salivary Gland Chromosomes of Sciara Compared with Normal	.75
26-178.	The Entire Salivary Gland Showing Nuclei (Sciara)	.75
26-179.	Salivary Gland Chromosomes in the Cell (Sciara)	.75
26-182.	Structure of Salivary Gland Chromosomes	.75
27-206.	Three Deficiencies of the X-Chromosome	.75
27-305.	Map of Salivary Gland Chromosomes of *D. Virilis*	.75
29-12.	Bridges' Revised Map of the S. G. X-Chromosome	.75
Set of 15 Lantern Slides		9.50
Any twelve of above lantern slides		8.00

† Volume and page number of illustration in the JOURNAL OF HEREDITY.

ILLUSTRATED LITERATURE

"Symposium Reprint" on Salivary Gland Chromosomes, containing articles from the JOURNAL OF HEREDITY, 1934-1938, by Painter, Bridges, Metz, Mackensen and Hughes—four Salivary Gland Chromosome maps, 15 illustrations, 54 pages $3.00

AMERICAN GENETIC ASSOCIATION

Victor Building **Washington, D. C.**